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AND

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"Nec aranearum sanè textus ideo melior quia ex se fila gignunt, nec noster vilior quia ex alienis libamus ut apes." JUST. LIPS. *Polit.* lib. i. cap. 1. Not.

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"Meditationis est perscrutari occulta; contemplationis est admirari perspicua . . . . . Admiratio generat quæstionem, quæstio investigationem, investigatio inventionem."—*Hugo de S. Victore.*

—"Cur spirent venti, cur terra dehiscat,
Cur mare turgescat, pelago cur tantus amaror,
Cur caput obscura Phœbus ferrugine condat,
Quid toties diros cogat flagrare cometas;
Quid pariat nubes, veniant cur fulmina cœlo,
Quo micet igne Iris, superos quis conciat orbes
Tam vario motu."

J. B. Pinelli ad Mazonium.

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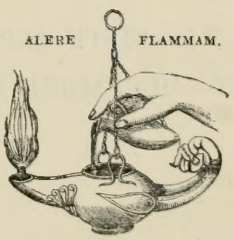

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ERRATA.

Page 89, line 3 from bottom, for $\frac{x}{a^2}\,dx + - \frac{y}{b^2}$ read $\frac{x}{a^2}dx + \frac{y}{b^2}$

— 90, — 13, for $x^{a^2} = cy^{b^2}$ read $x^{a^2} = Cy^{b^2}$

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[FIFTH SERIES.]

JULY 1878.

I. *On the Origin of Nebulæ.*

By JAMES CROLL, *LL.D., F.R.S.**

THE object of the present communication is to examine the bearings of the modern science of energy on the question of the origin of nebulæ, and in particular to consider the physical cause of the dispersion of matter into stellar space in the nebulous form. In doing so I have studiously avoided the introduction of mere hypotheses and principles not generally admitted by physicists. These remarks may be necessary, as the title of the paper might otherwise lead to the belief that it is on a speculative subject lying outside the province of the physicist.

The question of the origin of nebulæ is simplified by the theory, now generally received, that stars are suns like our own, and that nebulæ are in all probability stars in process of formation. The problem will therefore be most readily attacked by considering, first, the origin of our sun, as this orb, being the one most accessible to us, is that with which we are best acquainted.

By the origin of the sun I do not, of course, mean the origin of the matter constituting the sun—this being an inquiry with which the physicist has nothing whatever to do—but simply its origin as a sun, *i. e.* as a source of light and heat. Our first question must therefore be, What is the origin of the sun's heat? From what source did he derive that enormous amount of energy which in the form of heat he has been dis-

* Communicated by the Author.

sipating into space during past ages? Difficult as the question at first sight appears to be, it is yet simplified and brought within very narrow limits when we remember that there are only two conceivable sources. The sun must have derived his energy either from *Gravitation*, or from that other source to which I directed attention several years ago*, *Motion in Space.* All other sources of energy put together could not have supplied our luminary with one thousandth part of that which he has possessed. We are therefore compelled to attribute the sun's heat to one or other of these two, or to give up the whole inquiry as utterly hopeless. The important difference between the two is that the store of energy derivable from Gravitation could not possibly have exceeded 20 to 30 million years' supply of heat at the present rate of radiation; whereas the store derivable from Motion in Space, depending on the *rate* of that motion, may conceivably have amounted to any assignable quantity. Thus a mass equal to that of the sun, moving with a velocity of 476 miles per second, possesses in virtue of that motion energy sufficient, if converted into heat, to cover the present rate of the sun's radiation for 50 million years. Twice that velocity would give 200 million years' heat; four times that velocity would give 800 million years' heat, and so on without limit.

It is, however, not enough that we should have in the form of motion in space energy sufficient. We must have a means of converting this motion into heat—of converting motion of translation into molecular motion. To understand how this can be effected, we simply require the conception of *Collision.* Two bodies moving towards each other will have their motion of translation converted into molecular motion (heat) by their encounter.

To which of these two causes must we attribute the Sun's heat? It is certain that gravitation must have been a cause; and if we adopt the nebular hypothesis of the origin of our solar system, then from 20 to 30 million years' heat may thus be accounted for. But we know from geological evidence that the sun has been dissipating his light and heat at about the present rate for a much longer period. In a paper published in the 'Quarterly Journal of Science' for July 1877, I have discussed the geological evidence for the age of the earth at considerable length, and have pointed out that the time which has elapsed since life began on the globe cannot have been less than 60 million years. This estimate is based upon a rough estimate of the thickness of rock which has been removed by subaerial denudation since the earliest epoch of which geolo-

* Phil. Mag. May 1868.

gists take cognizance. Measuring the rate of the subaerial denudation by a method which I pointed out several years ago*, we are able to determine roughly the time required for the removal of the rock. But the 60 million years thus obtained, be it observed, are only the inferior limit. We know that a certain amount of rock has been removed; but how much more may have been carried away we cannot tell. Consequently, although we have good grounds for believing that 60 million years have elapsed since life began on the globe, yet the lapse of time may really have been very much longer. We are justified, therefore, in concluding that our globe has been receiving from the sun for the past 60 million years an amount of light and heat daily not very sensibly less than at present. This shows that gravitation alone will not explain the origin of the sun's heat, and that a far more effective cause must be found. Now the only other conceivable cause exceeding that of gravity is, of course, motion in space.

If the gravitation theory fails to explain the origin of the sun, it fails yet more decidedly to account for the nebulæ. In fact it does not attempt any explanation of the origin of the latter; for it begins by *assuming* their existence, and not only so, but that they are in *process of condensation*. This must be the case, because the theory in question assumes that the particles of a nebulous mass have, in virtue of gravity, a mutual tendency to approach one another; and it cannot tell us how this tendency could exist without producing its effect. The advocates of the theory are not at liberty to call in the aid of heat in order to explain why the particles are not mutally approaching; because it is this mutual approach which, according to the theory, produces the heat, and of course without such approach no heat could be generated. A nebulous mass with a tendency to condensation could not have existed from eternity as such; but what the previous condition of a nebula was, and how it came to assume its present state, the gravitation theory cannot say. It begins with a star or sun in process of formation, but does not help us to understand how the process of formation commenced.

It is quite otherwise, however, with the other theory. This latter does not, like the former, begin by assuming the existence of a nebulous mass; on the contrary, it goes back to the very commencement of physical inquiry, to the very point where physical investigation takes its rise, and beyond which we cannot penetrate. The only assumption it makes is that of the existence of matter and motion—if indeed this can be called an assumption. How matter and motion began to be,

* Phil. Mag. May 1868, and February 1867.

whether they were eternal or were created, are questions wholly beyond the domain of the physicist. The theory takes for a fact the existence of stellar masses in a state of motion; and its advocate is not required, as a physicist, to account for the existence either of those masses or of their motions. Neither is it necessary for him to advance any hypothesis to show how the masses came into collision; for unless we are to assume that all stellar masses are moving in one direction and with uniform velocity (a supposition contrary to known facts), then collisions must occasionally take place. The chances are that stellar masses are of all sizes, moving at random in all directions and with all velocities. We have here therefore, without any hypothesis, all the conditions necessary for the origin of nebulæ. Take the case of the origin of the nebulous mass out of which our sun is believed to have been formed. Suppose two bodies, each one half the mass of the sun, approaching each other directly at the rate of 476 miles per second (and there is nothing at all improbable in such a supposition), their collision would transform the whole of the motion into heat affording an amount sufficient to supply the present rate of radiation for 50 million years. Each pound of the mass would, by the stoppage of the motion, possess not less than 100,000,000,000 foot-pounds of energy transformed into heat, or as much heat as would suffice to melt 90 tons of iron or raise 264,000 tons 1° C. The whole mass would be converted into an incandescent gas, with a temperature of which we can form no adequate conception. If we assume the specific heat of the gaseous mass to be equal to that of air (viz. ·2374), the mass would have a temperature of about 300,000,000° C., or more than 140,000 times that of the voltaic arc.

Reason why Nebulæ occupy so much Space.—It may be objected that enormous as would be such a temperature, it would nevertheless be insufficient to expand the mass against gravity so as to occupy the entire space included within the orbit of Neptune. To this objection it might be replied, that if the temperature in question were not sufficient to produce the required expansion, it might readily have been so if the two bodies before encounter be assumed to possess a higher velocity, which of course might have been the case. But without making any such assumption, the necessary expansion of the mass can be accounted for on very simple principles. It follows in fact from the theory, that the expansion of the gaseous mass must have been far greater than could have resulted simply from the temperature produced by the concussion. This will be obvious by considering what must take place immediately after the encounter of the two bodies,

and before the mass has had sufficient time to pass completely into the gaseous condition. The two bodies coming into collision with such enormous velocities would not rebound like two elastic balls, neither would they instantly be converted into vapour by the encounter. The first effect of the blow would be to shiver them into fragments, small indeed as compared with the size of the bodies themselves, but still into what might be called in ordinary language immense blocks. Before the motion of the two bodies could be stopped, they would undoubtedly interpenetrate each other; and this of course would break them up into fragments. But this would only be the work of a few minutes. Here, then, we should have all the energy of the lost motion existing in these blocks as heat (molecular motion), while they were still in the solid state; for as yet they would not have had sufficient time to assume the gaseous condition. It is obvious, however, that the greater part of the heat would exist on the surface of the blocks (the place receiving the greatest concussion), and would continue there while the blocks retained their solid condition. It is difficult in imagination to realize what the temperature of the surfaces would be at this moment. For, supposing the heat were uniformly distributed through the entire mass, each pound, as we have already seen, would possess 100,000,000,000 foot-pounds of heat. But as the greater part of the heat would at this instant be concentrated on the outer layers of the blocks, these layers would be at once transformed into the gaseous condition, thus enveloping the blocks and filling the interspaces. The temperature of the incandescent gas, owing to this enormous concentration of heat, would be excessive, and its expansive force inconceivably great. As a consequence the blocks would be separated from each other, and driven in all directions with a velocity far more than sufficient to carry them to an infinite distance against the force of gravity were no opposing obstacle in their way. The blocks by their mutual impact would be shivered into smaller fragments, each of which would consequently become enveloped in incandescent gas. These smaller fragments would in a similar manner break up into still smaller pieces, and so on until the whole came to assume the gaseous state. The general effect of the explosion, however, would be to disperse the blocks in all directions, radiating from the centre of the mass. Those towards the outer circumference of the mass, meeting with little or no obstruction to their onward progress, would pass outwards into space to indefinite distances, leaving in this manner a free path for the layers of blocks behind them to follow in their track. Thus eventually a space, perhaps twice or even thrice that in-

cluded within the orbit of Neptune, might be filled with fragments by the time the whole had assumed the gaseous condition.

It would be the suddenness and almost instantaneity with which the mass would receive the entire store of energy, before it had time even to assume the molten, far less the gaseous condition, which would lead to such fearful explosions and dispersion of the materials. If the heat had been gradually applied, no explosions, and consequently no dispersion, of the materials would have taken place. There would first have been a gradual melting; and then the mass would pass by slow degrees into vapour, after which the vapour would rise in temperature as the heat continued until it became possessed of the entire amount. But the space thus occupied by the gaseous mass would necessarily be very much smaller than in the case we have been considering, where the shattered materials were first dispersed into space before the gaseous condition was assumed.

Reason why Nebulæ are of such various Shapes.—The latter theory accounts also for the various and irregular shapes assumed by the nebulæ; for although the dispersion of the materials would be in all directions, it would, according to the law of chances, very rarely take place uniformly in all directions. There would generally be a greater amount of dispersion in certain directions, and the materials would thus be carried along various lines and to diverse distances; and although gravity would tend to bring the widely scattered materials ultimately together into one or more spherical masses, yet, owing to the exceedingly rarefied condition of the gaseous mass, the nebulæ would change form but slowly.

Reason why Nebulæ emit such Feeble Light.—The feeble light emitted by nebulæ follows as a necessary result from the theory. The light of nebulæ is mainly derived from glowing hydrogen and nitrogen in a gaseous condition; and it is well known that these gases are exceedingly bad radiators. The oxyhydrogen flame, though its temperature is only surpassed by that of the voltaic arc, gives nevertheless a light so feeble as scarcely to be visible in the daytime. Now, even supposing the enormous space occupied by a nebula were due to excessive temperature, the light emitted would yet not be intense were it derived from nitrogen or hydrogen gas. The small luminosity of nebulæ, however, is due to a different cause. The enormous space occupied by those nebulæ is not so much owing to the heat which they possess, as to the fact that their materials were dispersed into space before they had time to pass into the gaseous condition; so that, by the time this latter state was assumed, the

space occupied was far greater than was demanded either by the temperature or the amount of heat received.

If we adopt the nebular hypothesis of the origin of our solar system, we must assume that our sun's mass, when in the condition of nebula, extended beyond the orbit of the planet Neptune, and consequently filled the entire space included within that orbit. Supposing Neptune's orbit to have been its outer limit, which it evidently was not, it would nevertheless have then occupied 274,000,000,000 times the space that it does at present. We shall assume, as before, that 50 million years' heat was generated by the concussion. Of course there might have been twice or even ten times that quantity; but it is of no importance what number of years is in the meantime adopted. Enormous as 50 million of years' heat is, it yet gives, as we shall presently see, only 32 foot-pounds for each cubic foot. The amount of heat due to concussion being equal, as before stated, to 100,000,000,000 foot-pounds for each pound of the mass, and a cubic foot of the sun at his present density of 1·43 weighing 89 lbs., each cubic foot must have possessed 8,900,000,000,000 foot-pounds. But when the mass was expanded to occupy 274,000,000,000 times more space, which it would do when it extended to the orbit of Neptune, the heat possessed by each cubic foot would then amount to only 32 foot-pounds.

In point of fact, however, it would not even amount to that; for a quantity equal to upwards of 20 million years' heat would necessarily be consumed in work against gravity in the expansion of the mass; all of which would, of course, be given back in the form of heat as the mass contracted. During the nebulous condition it would not exist as heat, so that only 19 foot-pounds out of the 32 foot-pounds generated by concussion would then exist as heat. The density of the nebula would be only $\frac{1}{16248160}$ that of hydrogen at ordinary temperature and pressure. The 19 foot-pounds of heat in each cubic foot would nevertheless be sufficient to maintain an excessive temperature; for there would be in each cubic foot only $\frac{1}{440000}$ of a grain of matter. But although the *temperature* would be excessive, the *quantity* both of light and heat in each cubic foot would of necessity be small. The heat being only $\frac{1}{71}$ of a thermal unit, the light emitted would certainly be exceedingly feeble, resembling very much the electric light in a vacuum-tube.

Heat and Light of Nebulæ cannot result from Condensation.—The fact that nebulæ are not only self-luminous but indicate the existence of hydrogen and nitrogen in an incandescent condition proves that they must possess a considerable tempe-

rature. And it is scarcely conceivable that the temperature could have been derived from the condensation of their masses. When our sun was in the nebulous condition it no doubt was self-luminous like other nebulæ, and doubtless would have appeared, if seen from one of the fixed stars, pretty much like other nebulæ as viewed from our earth. The spectrum would no doubt have revealed in it the presence of incandescent gas. At all events we have no reason to conclude that our nebula was in this respect an exception to the general rule, and essentially different from others of the same class. The heat which our nebula could have derived from condensation up to the time that Neptune was formed, no matter how far the outer circumference of the mass may originally have extended beyond the orbit of that planet, could not have amounted to over $\frac{1}{7000000}$ of a thermal unit for each cubic foot; and the quantity of light given out could not possibly have rendered the mass visible. Consequently the heat and light possessed by the mass must have been derived from some other source than that of gravity.

We have further evidence that the heat and light of nebulæ cannot have been derived from condensation. If there be any truth, as there doubtless is, in Mr. Lockyer's view of the evolution of the planets, then the nebulæ out of which these bodies were evolved must have originally possessed a very high temperature—a temperature so high, indeed, as to produce perfect chemical dissociation of the elements. In short, "the temperature of the nebulæ," as Mr. Lockyer remarks *, "was then as great as the temperature of the sun is now." Mr. Lockyer's theory is that the metals and the metalloids, owing to excessive temperature, existed in the nebulous mass uncombined—the metals, owing to their greater density, assuming the central position, and the metalloids keeping to the outside. The denser the metal the nearer would its position be to the centre of the mass, and the lighter the metalloid the nearer to the outside. As a general rule the dissociated elements would arrange themselves according to their densities; and it is for this reason, he considers, that the outer planets Neptune, Uranus, Saturn, and Jupiter, are less dense than the inner planets, since they must have been formed chiefly of metalloids, while the inner and more dense planets would consist chiefly of metallic elements.

"The hypothesis," says Mr. Lockyer, "is almost worthless unless we assume very high temperatures, because unless you have heat enough to give perfect dissociation, you will not have that sorting-out which always seems to follow the same

* 'Why the Earth's Chemistry is as it is," p. 55, 1877.

law." But the heat which produced this dissociation previous to the formation of the planets could not have been derived from the condensation of the nebula; for the quantity so derived prior to the existence of the outermost planet must have been infinitesimal indeed. The heat existing in the nebula previous to condensation must have come from some source; and we can conceive of no other save that which we have been considering.

The Gaseous State the first Condition of a Nebula.—If the foregoing be the true explanation of the origin of nebulæ, it will follow that the gaseous state will in most cases be the first or original condition, and that a nebula giving a continuous spectrum will only be found after it has condensed to a considerable extent.

The irresolvable nebulæ which exhibit bright lines, in all probability consist, as Mr. Huggins maintains, of glowing gas without any thing solid in them. In short they are nebulæ in their first stage of development, and have not as yet condensed sufficiently to become possessed of nuclei. If we adopt the generally accepted nebular hypothesis, I cannot understand how we can consistently deny the existence of gaseous nebulæ; for, according to the nebular hypothesis, the central nucleus which constitutes a sun or star, and which exhibits a continuous spectrum, was formed by condensation as surely as the planets or the satellites have been. Were we to go back sufficiently far in the past, we should come to a time when not only our globe but the sun himself consisted of gaseous matter only. If we admit this, then why not also admit that there may be nebulæ at the present time in a condition similar to what our sun must formerly have been.

The gaseous condition of nebulæ seems to follow as a consequence from Mr. Lockyer's theory. For in order that the materials in the formation of a sun or star may arrange themselves according to their densities, dissociation is requisite; but there can be no dissociation except in the gaseous condition.

Star-Clusters.—The wide-spread and irregular manner in which the materials would in many cases be distributed through space after collision, would prevent a nebula from condensing into a single mass. Subordinate centres of attraction, as was long ago shown by Sir William Herschel (in his famous memoir on the formation of stars*), would be established, around which the gaseous particles would arrange themselves and gradually condense into separate stars, which would finally assume the condition of a cluster.

* Phil. Trans. for 1811.

Binary, Triple, and Multiple systems of stars will of course be accounted for in a similar manner.

It is conceivable that it may sometimes happen that by the time the materials are broken up and dissipated into space, there may not be sufficient heat left to convert the fragments into vapour. In this case we should have what Professor Tait has suggested, a nebula consisting of "clouds of stones." But such nebulæ must be of rare occurrence.

Objections considered.—On a former occasion I considered one or two anticipated objections to the theory that stellar light and heat were derived from motion in space. But as these objections have since been repeatedly urged by physicists both in this country and in America, I shall again briefly refer to them.

Objection 1st. "The existence of such non-luminous bodies as the theory assumes is purely conjectural, as no such bodies have ever been observed." In reply, it is just as legitimate an inference that there are bodies in stellar space not luminous as that there are luminous bodies in space not visible. We have just as good evidence for believing in the existence of the one as we have in the existence of the other. Bodies in stellar space can only be known through the eye to exist. If they are not luminous, they of course cannot be seen. But we are not warranted on that account to suppose that they do not exist, any more than we have to suppose that stars do not exist which are beyond the reach of our vision. We have, however, positive evidence that there are bodies in space non-luminous, as the meteorites and planets for example. The stars are beyond doubt suns like our own; and we cannot avoid the inference that, like our sun, they are surrounded by planets. If so, then we have to admit that there are far more bodies in stellar space non-luminous than luminous. But this is not all: the stars no more than our sun can have been dissipating their light and heat during all past ages; their light and heat must have had a beginning; and before that they could not be luminous. Neither can they continue to give out light and heat eternally; consequently when their store of energy is exhausted they will be non-luminous again. Light and heat are not the permanent possession of a body. A body may retain its energy in the form of motion undiminished and untransformed through all eternity, but not so in the form of heat and light. These are forms of energy which are being constantly dissipated into space and lost in so far as the body is concerned.

The conclusion to which we are therefore led is that there are in all probability bodies in stellar space which have not

yet received their store of light and heat, while there are others which have entirely lost it. The stars are probably only those stellar masses which having recently had an encounter have become possessed of light and heat. They have gained in light and heat what they have lost in motion, but they have gained a possession which they cannot retain, and when it is lost they become again what they originally were —dark bodies.

2nd. "We have no instances of stellar motions comparable with those demanded by the theory." A little consideration will show that this is an objection which, like the former, can hardly be admitted. No body of course moving at the rate of 400 miles per second could remain a member of our solar system; and beyond our system the only bodies visible are the nebulæ and fixed stars; and they are according to the theory visible because like the sun they have lost their motion—the lost motion being the origin of their light and heat. Their comparatively small velocities are in reality evidence in favour of the theory than otherwise; for had the stars been moving with excessive velocities this would have been adduced as proof that their light and heat could not have been derived from motion lost, as the theory assumes.

3rd. "If suns or stars have been formed by collision of bodies moving in space, proper motion can be none other than the unused and unconverted energy of the original components. And as stellar bodies are likely of all sizes and moving with all manner of velocities, it must often happen, from the unequal force of the impinging masses, that a large proportion of the original motion must remain unconverted into heat. Consequently some of the stars ought, according to the theory, to possess great velocities—which is not the case, as none of the stars have a motion of more than 30 or 40 miles per second."

I freely admit that, if it could be proved that none of the stars have a proper motion of more than 30 or 40 miles per second, it would at least be a formidable difficulty in the way of accepting the theory. For it would indeed be strange that amidst all the diversity of dimensions of heavenly bodies, it should invariably happen that the resultant movement of the combined masses should be reduced to such comparatively insignificant figures. But something more definite must yet be known in reference to the motion of the stars before this objection can be urged.

All that we are at present warranted to assume is simply that, of the comparatively few stars whose rate of motion has been properly measured, none have a greater velocity than 30 or 40 miles per second, while nothing whatever is known

with certainty as to the rate of motion of the greater number of the stars.

There seems to be a somewhat prevailing misapprehension regarding the extent of our knowledge of stellar motions. Before we can ascertain the rate of motion of a star from its angular displacement of position in a given time, we must know its absolute distance. But it is only of the few stars which show a well-marked parallax that we can estimate the distance; for it is now generally admitted that there is no relation between the apparent magnitude and the real distance of a star. All that we know in regard to the distances of the greater mass of the stars is little else than mere conjecture. Even supposing we knew the absolute distance of a star and could measure its amount of displacement in a given time, still we could not be certain of its rate of motion unless we knew that it was moving directly at right angles to the line of vision, and not at the same time receding or advancing towards us; and this we could not determine by mere observation. The rate of motion, as determined from its observed change of position, may be, say, only twenty miles a second, while its actual velocity may be ten times that amount.

By spectrum-analysis it is true we can determine the rate at which a star may be advancing or receding along the line of sight independently of any knowledge of its distance. But this again does not give us the actual rate of motion, unless we are certain that it is moving directly to or from us. If it is at the same time moving transversely to the observer, its actual motion may be more than a hundred miles per second, while the rate at which it is receding or advancing, as determined by spectrum-analysis, may not be 20 miles a second. But in many cases it would be difficult to ascertain whether the star had a transverse motion or not. A star, for example, 1000 times more remote than α Centauri (that is, twenty thousand billion miles), though moving transversely to the observer at the enormous rate of 100 miles per second, would take upwards of 30 years to change its position so much as $1''$, and 1800 years to change its position $1'$; in fact we should have to watch the star for a generation or two before we could be certain whether it was changing its position or not. And even after we had found with certainty that the star was shifting, and this at the rate of $1'$ in 1800 years, we could not, without a knowledge of its distance, express the angle of displacement in miles. But from the apparent magnitude or brilliancy of the star, we could not determine whether its distance was 10 times, 100 times, or 1000 times that of α Centauri; and consequently we could form no con-

jecture as to the actual velocity of the star. If we assumed its distance to be 10 times that of α Centauri, this would give a transverse velocity of one mile per second. If we assumed its distance to be 100 times that of α Centauri, this would give 10 miles a second as the velocity, and if 1000 times, the velocity of course would be 100 miles per second.

As there are but few of the stars which show a measurable parallax, and we have no other reliable method of estimating their distances*, it follows that in reference to the greater number of the stars, neither by spectrum-analysis nor by observation of their change of position can we determine their velocities. There does not, therefore, appear to be the shadow of a reason for believing that none of the stars has a motion of over 30 or 40 miles per second: for any thing that at present is known to the contrary, many of them may possess a proper motion enormously greater than that.

There is, however, an important point which seems to be overlooked in this objection, viz. that, unless the greater part of the motion of translation be transformed into heat, the chances are that no sun star will be formed. It is necessary to the formation of a sun which is to endure for millions of years, and to form the centre of a planetary system like our own, that the masses coming into collision should be converted into an incandescent nebulous mass. But the greater the amount of motion left unconverted into heat, the less is the chance of this condition being attained. A concussion which would leave the greater part of the motion of translation untransformed, would be likely as a general rule to produce merely a temporary star, which would blaze forth for a few years, or a few hundred years, or perhaps a few thousand years and then die out. In fact we have had several good examples of such since the time of Hipparchus. Now, although it may be true that, according to the law of chances, collisions producing temporary stars must be far more numerous than those resulting in the formation of permanent stars, nevertheless the number of those temporary stars observable in the heavens may be perfectly insignificant in comparison with the number of permanent stars. Suppose there were as many as one hundred temporary stars formed for one permanent, and that on an average each should continue visible for 1000 years, there would not at the present moment be over half-a-dozen of such stars visible in the heavens.

4th. "Such collisions as the theory assumes are wholly

* It is true that we may one day be able to determine by spectrum-analysis the distance of some of the binary stars; but as yet this method has not been applied with success.

hypothetical; it is extremely improbable that two cosmical bodies should move in the same straight line; and of two moving in different lines, it is improbable that either should impinge against the other." In reply, if there are stellar masses moving in all directions, collisions are unavoidable. It is true they will be of rare occurrence: but it is well that it is so; for if they had been frequent the universe would be in a blaze, and its store of energy soon converted into heat.

II. *On the Analysis of Alloys containing Copper, Zinc, and Nickel.* *By* Thomas Bayley, *Assoc. R.C.Sc.I.**

THE analysis of these alloys can be very rapidly effected by a combination of colorimetric and volumetric methods. The alloy is dissolved in nitric acid, and the solution then evaporated to dryness with excess of sulphuric acid to expel nitric acid, which must not be left in the solution.

Determination of the Copper.—The solution is mixed with excess of potassic iodide, which causes the formation of cuprous iodide, according to the following well-known reaction:—

$$2CuSO_4 + 4KI = 2K_2SO_4 + Cu_2I_2 + I_2.$$

The solution containing the precipitate is then titrated with a standard solution of sodic thiosulphate. The free iodine present is an exact measure of the copper, each gram of copper being equal to two grams of iodine. The following is the result of a series of determinations of copper made under various circumstances by this method:—

Cu taken.	Iodine.
·0635 Cu	·1275
·1250 Cu (free HCl present)	·2500
·2531 Cu	·5100
·1408 Cu (free H_2SO_4 present)	·2740
·1408 Cu ($NiSO_4$ present)	·2755
·1408 Cu ($ZnSO_4$ and free H_2SO_4 present)	·2760

The solution, after the titration, is filtered and the precipitate washed. The filtrate is free from copper and contains the nickel and zinc. The absence of copper was proved in several experiments by evaporating to dryness and gently heating the residue, after which it was dissolved in a little dilute sulphuric acid and excess of ammonia added. In no instance was any blue colour perceptible.

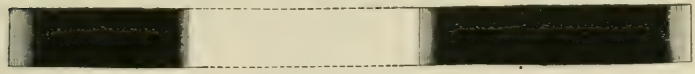
Determination of the Nickel.—The fact will have been observed by chemists, that solutions of nickel and cobalt salts

* Communicated by the Author.

are so far complementary in colour, that when they are mixed together the resulting liquid, if moderately dilute, is hardly to be distinguished from pure water. I conceived this fact might be made the basis of a method for estimating nickel and cobalt, and therefore undertook the following experiments.

A large hollow prism filled with a moderately strong solution of a nickel or cobalt salt was placed immediately in front of the slit of the spectroscope; and the thickness of the liquid traversed by the light was regulated by moving the prism until the eye could most clearly determine the dark absorption-band caused by the metal in solution. On referring to the accompanying diagrams, which show the absorption-spectra of the two metals, it will be seen that cobalt and nickel are almost exactly complementary in their relations to light. The black band of cobalt is well defined at the edges, especially at the end nearest to the red; while the absorption-bands of nickel are not so sharply defined, but fade away at each end. If the

Spectrum of light passed through Co.



Spectrum of light passed through Ni and Co.

Spectrum of light passed through Ni.

The black parts here represent the bright parts of the spectrum, and *vice versâ*.

spectra were exactly complementary, on superimposing the nickel spectrum upon the cobalt spectrum, the dark part on the one would exactly cover the light part on the other. This, however, though nearly the case, is not exactly so; for the bright band in the nickel spectrum overlaps the dark cobalt band at the end nearest to the red, although with diminished brilliancy. Consequently, when we employ a mixture of nickel and cobalt salts in solution, we do not get a uniformly dark spectrum, but an excess of light coming through at the part where the overlapping occurs, as seen in the diagram. This is why the solution obtained by mixing strong solutions of nickel and cobalt is not grey, but reddish brown in colour.

Having so far demonstrated the complementary character of the two metals, I next endeavoured to find in what proportions they must be mixed in order to neutralize each other. For this purpose a tall glass cylinder (150 cubic centims. capacity),

in which ammonia is estimated by Nessler's method, was employed. Dilute standard solutions of pure nickel and cobalt having been carefully prepared, a measured quantity of cobalt solution was placed in the cylinder, and the nickel added from a burette until the neutral point was reached. It is difficult by this method to distinguish the exact point of neutrality, but easy to determine that the colour-coefficient of nickel with regard to cobalt lies between 3·1 and 3·2. That is to say, if a quantity of cobalt in solution be mixed with a solution containing 3·1 times its weight of nickel, the cobalt colour will slightly predominate in the mixture, which will have a reddish tinge; while if a solution containing 3·2 times its weight of nickel be added, the nickel colour will be slightly in excess and the solution will have an olive-green tinge. It is only with dilute solutions containing not more than about 2·5 grams of the metals per litre that it is possible to determine the coefficient with this accuracy.

I now sought for some method of indicating more exactly the neutral point. After several attempts, it was found that the addition of ammonium carbonate to the solution of the two metals affords a means of determining whether the slightest excess of either metal is present.

If we take 25 cubic centims. of solution containing ·03125 gram of Co, and add to this 39·25 cubic centims. of solution containing ·098125 gram of nickel, the resulting liquid appears perfectly colourless. If we now dilute the mixed solutions to 100 cubic centims., and transfer 25 cubic centims. of that solution, containing ·0078125 gram of cobalt and ·02453125 gram of nickel, to a tall glass jar, add 25 cubic centims. of the solution of ammonium carbonate, described hereafter, and then dilute to 150 cubic centims., the result is a liquid of deep purple colour. If we repeat this experiment, using in the first instance ·03125 gram of cobalt and ·099375 gram of nickel, the colour of the 150 cubic centims. is not purple, but of a distinct blue colour. The ammonium carbonate for this purpose must be neutral, as the excess of either base or acid destroys the delicacy of the reaction.

The solution of neutral carbonate, $(NH_4)_2CO_3$, was prepared as follows:—A few ounces of the commercial carbonate having been dissolved in water, 10 cubic centims. of the solution were neutralized by standard solution of sulphuric acid. The quantity of NH_3 in the 10 cubic centims. was found to be ·085 gram. The quantity of CO_2 in an equal quantity of the solution was found to be, in two experiments, ·348 gram and ·350 gram (mean ·349 gram): the amount of CO_2 required to form the neutral carbonate with ·085 gram of NH_3 being ·110, it

follows that there was an excess of CO_2 equal to ·259 gram in every 10 cubic centims. of the original solution of commercial carbonate. To neutralize this, 18 grams of ammonia were required to be added to a litre of the commercial carbonate solution. This was furnished by 61·7 cubic centims. of ammonia solution (of sp. gr. ·880).

I next endeavoured to determine whether the nature of the salt of nickel or cobalt has any effect on the reaction. For this purpose the following solutions were prepared:—

$CoCl_2$ . .	1 cubic centim.	= ·00125	gram Co.
$NiCl_2$ . .	1 cubic centim.	= ·0025	„ Ni.
$Ni(NO_3)_2$.	1 cubic centim.	= ·0025	„ Ni.
$NiSO_4$ . .	1 cubic centim.	= ·0025	„ Ni.
$Co(NO_3)_2$.	1 cubic centim.	= ·00125	„ Co.
$CoSO_4$ . .	1 cubic centim.	= ·00125	„ Co.

The method of proceeding was as follows:—Into each of five cylinders 25 cub. centims of the standard solution of cobaltous chloride were placed; to the first cylinder 39 cubic centims. of the solution of nickelous chloride were added, to the second cylinder 39·25 cubic centims., and so on, to the fifth cylinder 40 cubic centims. of nickelous chloride being added. Each cylinder was then made up to 100 cubic centims., and 25 cubic centims. out of each 100 cubic centims. placed in a second series of cylinders. To each of the second series neutral ammonium carbonate (25 cubic centims.) was added, and then sufficient water to make 150 cubic centims. The results are expressed in the following Table:—

Cylinder.	Co used.	Ni used.	Colour.	Ratio of Ni to Co.
1.	·03125 grm.	·097500 grm.	Purple	3·12
2.	·03125 „	·098125 „	Slightly purple.	3·14
3.	·03125 „	·098750 „	Between 2 and 4.	3·16
4.	·03125 „	·099375 „	Slightly blue	3·18
5.	·03125 „	·100000 „	Blue.	3·20

In two experiments, using in the first solutions of $CoCl_2$ and $NiSO_4$ and in the second solutions of $CoCl_2$ and $Ni(NO_3)_2$, I obtained exactly the same results; so that the foregoing Table expresses the results of these experiments. Subsequently experiments were made with the same quantities of the metals in the following combinations:—

$Co(NO_3)_2$ with $NiSO_4$, $Ni(NO_3)_2$ and $NiCl_2$,
$CoSO_4$ with $Ni(NO_3)_2$, $NiSO_4$ and $NiCl_2$.

The results of these latter experiments were exactly the same as those of the first experiments, so that the Table also does equally well to express them.

If the cylinders, after the addition of the ammonium carbonate be allowed to stand, the differences of tint disappear in a few hours, and a uniform deep purple-red tint is produced. This is caused by the cobalt absorbing oxygen from the air to form the double compounds of cobalt and ammonia. A small quantity of a sulphite destroys the reaction, as it changes the tint to a deep brown. Thiosulphates and some other reducing-agents do not act in this way.

These experiments lead to the conclusion that the colour-coefficient of nickel with regard to cobalt is 3·16 in all cases, or, in other words, that the tint of nickel and cobalt solutions is independent of the acid radical in combination with the metals, and depends only upon the metal in solution.

It is evident that nickel and cobalt may be estimated by means of this reaction. As an example of its application to this purpose, I give the following account of the manner in which small quantities of nickel may be estimated.

The nickel must be dissolved in an acid and the solution diluted to any convenient quantity, *e. g.* 50 or 100 cubic centims. Into each of three cylinders ·0078125 grm. of Co as $CoCl_2$ is placed. This amount of cobalt is afforded by 6·25 cub. centims. of the standard $CoCl_2$ solution. Calling the cylinders No. 1, No. 2, and No. 3, we place in No. 1 ·024531 grm. of nickel in solution, and in No. 3 ·0248458 grm. To the three cylinders we then add 25 cubic centims. of the standard ammonium carbonate. Cylinder No. 2, which contains only cobalt solution and ammonium carbonate, is then made up nearly to 150 cub. centims., and No. 1 and No. 3 are filled up to that quantity. Cylinder No. 1 has then a purple tinge, while cylinder No 3 has a blue tinge. By adding from a burette the solution whose strength we wish to determine to No. 2 until its tint is intermediate between No. 1 and No. 3, we make with great accuracy the required determination. In all cases the cylinders should be held, whilst under comparison, with their lower extremities at some inches distance above a sheet of white paper. Three experiments that by no means reached the highest limit of accuracy, gave the following results :—

Ni in solution.	Ni found.
·02469 grm.	(1) ·02425
,, ,,	(2) ·02475
,, ,,	(3) ·02500
	·02466 = mean.

It is evident that a similar plan of estimating cobalt would be still more accurate on account of the higher colour-efficiency of that metal.

The partially opaque brown solution obtained by mixing *strong* solutions of nickel and cobalt might, I think, be used for making standards for the purposes of colorimetrical analysis. For instance, the brown solution mixed with a few drops of potassic bichromate cannot be distinguished from Nesslerized ammonia. Probably the tests used to compare the solutions of steel in Eggertz's process for the estimation of carbon might be made in a similar manner. They would have the advantage of being permanent.

After the determination of the copper and nickel by the above methods, the zinc is obtained by difference. If lead is present in the alloy, it is separated by the evaporation with sulphuric acid.

III. *On Picoline and its Derivatives.* *By* WILLIAM RAMSAY, PH.D., *Tutorial Assistant of Chemistry in the Glasgow University.*

IN my last memoir (Phil. Mag. Oct. 1877, p. 251) I mentioned that on oxidizing lutidine with potassium permanganate an acid, or a mixture of acids, was obtained, the silver salt of which contained 45·82 per cent. of silver, corresponding to a molecular weight of 257, assuming the acid to be dibasic. Since that time I have ascertained that the silver salt of the acid giving the above percentage of silver is not a pure compound, but a mixture of an acid and a neutral salt of at least two acids of the same formula.

Oxidation of Lutidine.—About 100 grams of lutidine, C_7H_9N, boiling from 152° to 155° C., were oxidized with potassium permanganate in a tinned iron vessel in precisely the same manner as that described for picoline. After complete oxidation the manganese oxide was removed by filtration, and the filtrate was distilled to recover unoxidized lutidine; less had escaped oxidation than was the case with picoline, probably on account of its higher boiling-point, as well as of its being more easily oxidized than its lower homologue. The highly alkaline solution of the products of oxidation was then evaporated to dryness, and digested with absolute alcohol by means of an apparatus which permitted the hot alcohol continually to drop on the mixture of salts. After three or four days the insoluble residue consisted of nearly pure potassium carbonate, the organic salts having dissolved in the alcohol. The alcohol was then removed by evaporation. The residue had an extremely persistent bitter taste, almost comparable with that of strychnine. It was

* Communicated by the Author.

dissolved in water, neutralized, and precipitated while hot with lead nitrate, less being used than was necessary to combine with all the acid present. The precipitate was white and flocculent. When decomposed with sulphuretted hydrogen it yielded a liquid from which, on concentration, dicarbopyridenic acid separated out in its usual form of hair-like needles, and when purified by crystallization from water was obtained in its two other crystalline forms, viz. naphthalene-like plates and short thick crystals. Its purest form, the thick crystals were converted into the silver salt, after determination of the water of crystallization. It contained 10·94 per cent. of water; and the silver salt, on ignition, was found to contain 56·57 per cent. of silver. Dicarbopyridenic acid, in the same crystalline form, crystallizes with 9·83 per cent., or one molecule of water; and its silver salt contains 56·69 per cent. of silver. The silver salt was almost completely insoluble in boiling water, and was gelatinous and very bulky. The filtrate from a boiling solution deposited a few *amorphous* flocks on cooling.

The calcium salt, prepared by addition of calcium chloride to a hot solution of the ammonium salt, crystallized in thin plates, usually grouped together in masses. It contained 17·47 per cent. of water, which it lost at 150°, and the anhydrous salt 19·96 per cent. of calcium. $C_7H_3NO_4Ca \cdot 2\frac{1}{2}H_2O$ requires 18·00 per cent. of water, and the anhydrous salt 19·51 per cent. of calcium. I had not obtained the salt crystallized with water before; that described in my last memoir was anhydrous. On evaporation of the solution of the calcium salt, needles were precipitated which were anhydrous and on analysis yielded 19·01 per cent. of calcium. The formula of this acid is certainly $C_7H_5NO_4$; and from its crystalline forms it appears to be identical with the dicarbopyridenic acid discovered by Professor Dewar. The yield was somewhat more than ten grams. Whether it is derived from lutidine by oxidation, or from a possible impurity of picoline in the lutidine, I am unable to decide.

The mother liquor of the dicarbopyridenic acid contained a mixture from which an acid crystallizing in groups of acicular crystals radiating from a common centre was deposited, but not in sufficient quantity for complete examination. From consideration of its crystalline form it is probably identical with one shortly to be described.

The remainder of the solution of potassium salts, from which the dicarbopyridenic acid was precipitated in combination with lead, was treated with excess of lead nitrate. A copious flocky precipitate came down, from which the acids were

liberated as usual by treatment with sulphuretted hydrogen. The lead sulphide was well washed with boiling water; the filtrate and washings were evaporated; and on cooling, a white substance deposited mixed with spear-like crystals. After a long series of fractional crystallizations from water three distinct acids were obtained, of which I have investigated two; the third was unfortunately lost. As these two have the same percentage composition as Dewar's dicarbopyridenic acid, I propose to name that acid *α-dicarbopyridenic acid.*

The white substance, mentioned above, consisted chiefly of a sparingly soluble acid, which I shall call *β-dicarbopyridenic acid.*

The acid depositing from the mother liquor of the β-acid, in groups of spear-like crystals, shall be distinguished as *γ-dicarbopyridenic acid.*

β-Dicarbopyridenic acid, $C_7H_5NO_4$.—This acid, as has already been mentioned, separates out as a white crystalline powder when its aqueous solution is allowed to cool. It was purified by repeated crystallization from water. When viewed under the microscope it presents the appearance of truncated octahedra, grouped together in masses. The acid dried at 100° decomposes at 244–245°, frothing up, and giving off gas. It is very sparingly soluble in water; 0·2 gram, dissolved by boiling with about 200 cubic centimetres of water, crystallized out after standing for some hours. It is rather more soluble in alcohol, and dissolves sparingly in ether. When heated on platinum-foil it melts, effervesces, and gives off a smell resembling that of pyridine. It was analyzed, with the following results:—

I. Taken, 0·4012 gram of air-dried acid. Loss at 100°, 0·0532 gram; CO_2 obtained on combustion, 0·6611 gram; H_2O, 0·1045 gram.

II. Taken, 0·4233 gram of air-dried acid. Loss at 100°, 0·0560 gram; CO_2, 0·6935 gram; H_2O, 0·1025 gram.

	I.	II.	Calculated for $C_7H_5NO_4 . 1\frac{1}{2}H_2O$.
H_2O	13·26 per cent.	13·20 per cent.	13·91 per cent.
			For $C_7H_5NO_4$.
C	51·81 ,,	51·49 ,,	50·30 per cent.
H	3·33 ,,	3·10 ,,	3·00 ,,

To ascertain if this acid, when heated, splits up into carbonic anhydride and pyridine, thus,

$$C_7H_5NO_4 = C_5H_5N + 2CO_2,$$

its vapour-density was taken at the boiling-point of sulphur (446° C.) by means of the beautiful arrangement devised by

Victor Meyer, and described in the *Berichte der deutschen chemischen Gesellschaft*, ix. p. 1216.

Results. Taken, 0·0151 gram.

Weight of empty bulb..... 11·6 grams.
Weight of bulb full of alloy... 199·6 „
Weight of bulb after heating. 58·3 „
Height of barometer ..... 750 millims.
Height of alloy in tube above level in bulb..... 60 „

The formula given by Meyer (*Berichte*, 1877, p. 2070),

$$D=\frac{S\,.\,1543500}{\left[P+\frac{2}{3}p\right]\left[\left(\frac{a}{9\cdot608}\right)1\cdot01-\frac{r}{9\cdot158}\right]},$$

where D is the vapour-density compared with air;
S the weight of substance taken;
P the height of the barometer;
p the height of alloy in tube, above level in bulb;
a the amount of alloy used;
9·608 the spec. grav. of the alloy at 100°;
9·158 „ „ „ 446°;
r the weight of alloy remaining in the bulb after the operation is over.

Substituting the numbers found,

$$D=\frac{0\cdot0151\times1543500}{(750+40)\left\{\left(\frac{188\cdot0}{9\cdot608}\right)1\cdot01-\left(\frac{46\cdot7}{9\cdot158}\right)\right\}}=2\cdot06,$$

or, compared with hydrogen, 29·22. The molecular weight of C_5H_5N is 79, and of $2CO_2$ 88. Adding these together and dividing by 6 we get 27·8. It is therefore clear that the above equation is correct, and that β-dicarbopyridenic acid decomposes into pyridine and carbonic anhydride at the boiling-point of sulphur.

Reactions of β-Dicarbopyridenic acid.—A cold saturated solution gave no precipitate with the following salts:—$CaCl_2$, $BaCl_2$, BaH_2O_2, $MgSO_4$, $ZnSO_4$, $CdCl_2$, $Ni(NO_3)_2$, $Co(NO_3)_2$, $HgCl_2$, alum, chrome-alum, $MnCl_2$, $SnCl_4$. With ferric chloride it gave a white flocculent precipitate soluble in hydrochloric acid; the precipitate, on addition of ammonia, turned reddish-brown in colour, and apparently changed to ferric hydrate. With mercurous nitrate it gives a bulky white precipitate; with copper sulphate a light blue precipitate, turning darker on drying; with lead nitrate a heavy white crystalline precipitate after some time; and with silver nitrate white crystalline flocks very sparingly soluble in water.

Ferrous sulphate gives a red coloration exactly resembling that produced by α-dicarbopyridenic acid.

Salts of β-Dicarbopyridenic acid.

Ammonium salt.—This salt was prepared by evaporating the acid to dryness with aqueous ammonia. It is moderately soluble, and crystallizes from a hot solution in needles.

Synthesis. Taken, 0·4025 gram of acid.

Gain, on evaporation with ammonia, 0·0925 gram,
=18·68 per cent. NH_3. Calculated, 16·91 per cent.

Calcium salt.—Prepared by adding calcium chloride to a hot solution of the ammonium salt. It forms very thin microscopic needles, sparingly soluble in water.

Analysis. Taken, 0·3190 gram.

Loss at 150°, 0·0483 gram, =15·14 per cent. H_2O.
$CaCO_3$ ..... 0·1320 gram, =16·55 per cent. Ca.

Calculated for $C_7H_3NO_4Ca \cdot 2H_2O$ = 14·93 per cent. H_2O.
16·91 per cent. Ca.

Iron salt.—The white flocculent precipitate obtained by adding ferric chloride to the acid was analyzed, after being dried at 150°.

Taken, 0·1020 gram.

Fe_2O_3, 0·0304 gram, =0·0213 gram Fe, or 20·86 per cent.
Calculated for $(C_7H_3NO_4)_3Fe_2$ ... Fe, or 18·45 per cent.

The salt is probably basic. On addition of ammonia to the moist salt it is converted into ferric hydrate.

Lead salt.—A white crystalline precipitate, very sparingly soluble in water, prepared from the ammonium salt with lead nitrate.

Taken ..... 0·4165 gram.
Loss at 150° 0·0367 gram, = 8·81 per cent. H_2O.
Lead sulphate 0·3117 gram, =51·14 per cent. Pb.
Calculated for $C_7H_3NO_4Pb \cdot 2H_2O$, 8·82 per cent. H_2O.
50·73 per cent. Pb.

Silver salt.—The silver salt of β-dicarbopyridenic acid, when precipitated by adding silver nitrate to a solution of the acid, contains 43·56 per cent. of silver, and is probably a mixture of neutral and acid salt. It was this which led me, in my last paper (Phil. Mag. Oct. 1877), to state that the molecular weight of the acids from lutidine was higher than that of the picoline acids. Two analyses of the silver salt prepared in this manner showed it to contain 43·56 per cent. and 44·05 per cent. of silver. When prepared by adding silver nitrate to the ammonium salt, it has the formula $C_7H_3NO_4Ag_2$, as the following analyses prove:—

I. Taken...0·3247 gram.
Silver...0·1825 gram, = 56·23 per cent.
II. Taken...0·3590 gram.
Silver...0·2031 gram, = 56·57 per cent.
$C_7H_3NO_4Ag_2$ contains 56·69 per cent. of silver.

This silver salt is not nearly so bulky as that of α-dicarbopyridenic acid, but comes down in flocks. It is very sparingly soluble in hot water, and crystallizes out on cooling in tufts of microscopic needles. When heated it evolves the usual smell of pyridine. It does not blacken on exposure to light.

Methyl ether.—Prepared by cohobating the silver salt with methyl iodide. It is a very deliquescent white solid, crystallizing in needles, and soluble in water, alcohol, and ether.

Chloride.—One gram of β-dicarbopyridenic acid was distilled from a small retort along with 2·7 grams of phosphoric chloride. The mixture liquefied before heat was applied. The distillate was fractionated. After the oxychloride had distilled over at 110° the thermometer rose rapidly to 269–270°, and remained stationary while the chloride distilled. The distillate solidified to a mass of white needles, melting at 49°, and remaining liquid long after the ordinary temperature had been regained.

γ-Dicarbopyridenic acid, $C_7H_5NO_4$.—γ-Dicarbopyridenic acid is much more soluble then the β-acid, and crystallizes out from the mother liquor of the latter. After repeated recrystallization, to free it from the β-acid, and from a still more soluble bitter substance, it was obtained in a pure state. It forms tufts of spear-like crystals springing from a common nucleus. When its solution in hot water is allowed to cool, it crystallizes from nuclei which form on the surface of the liquid. After being dried by exposure to air it has a satin-like lustre. It is moderately soluble in water at 18°; 100 cubic centimetres of its saturated solution left, on evaporation to dryness, 1·1580 gram of dry acid. It is easily soluble in alcohol and ether. Like its two isomerides, it decomposes when heated into pyridine and carbonic anhydride about 241–245°. To ascertain if it decomposed in the same manner as the β-acid, I determined the vapour-density of the mixture of pyridine and carbonic anhydride yielded by the acid at the boiling-point of sulphur. To save space I shall omit the details of the determination, and shall merely state that the number found was 28·4, compared with hydrogen. The number required by the equation

$$C_7H_5NO_4 = C_5H_5N + 2CO_2$$

is 27·8. The equation is thus proved to represent what happens when γ-dicarbopyridenic acid is decomposed by heat.

γ-Dicarbopyridenic acid gave the following numbers on analysis:—

I. Taken ..... 0·3075 gram.
Loss at 100°, 0·0475 gram, = 15·44 per cent. H_2O.

The remainder, 0·2600 gram, on combustion gave
CO_2 ..... 0·4929 gram, = 51·70 per cent. C.
H_2O ..... 0·0740 gram, = 3·16 per cent. H.

II. Taken ..... 0·3582 per cent. of dry acid.
CO_2 ..... 0·6697 gram, = 50·98 per cent. C.
H_2O ..... 0·0974 gram, = 3·02 per cent. H.

III. When dried for one night over sulphuric acid, 0·3085 gram of the acid lost at 100° 0·0380 gram, = 12·31 per cent. of H_2O.

IV. After being dried for a long time in air, 0·2290 gram lost at 100° 0·0235 gram, = 10·26 per cent. H_2O.

$C_7H_5NO_4$ contains 50·30 per cent. C. and 3·00 per cent. H. $C_7H_5NO_4 . H_2O$ contains 9·73 per cent. H_2O; with $1\frac{1}{2}H_2O$, 13·91 per cent.; and with $2\,H_2O$, 17·78 per cent.

The acid, when dried in air, appears to contain less than two molecules of water, and when dried over sulphuric acid, more than one molecule.

Reactions of γ-Dicarbopyridenic acid.—A cold saturated solution of the acid gave no precipitate with the following salts:—Ca Cl_2, even after addition of ammonia; Ba Cl_2, $MgSO_4$, Zn SO_4, Cd Cl_2, Hg Cl_2, Ni $(NO_3)_2$, Co$(NO_3)_2$. Ferric chloride causes a white flocculent precipitate, soluble in hydrochloric acid, and converted into ferric hydrate by ammonia; mercurous nitrate gives a bulky white precipitate; stannous chloride, a flocculent precipitate; copper sulphate, a whitish blue precipitate; lead nitrate, a white precipitate, which disappears at first, probably owing to formation of an acid salt, but on addition of more lead salt it becomes permanent; silver nitrate gives a white flocculent precipitate, slightly soluble in hot water. Ferrous sulphate gives a blood-red coloration.

Salts of γ-Dicarbopyridenic acid.

Ammonium salt.—White needles.

0·2708 gram of acid gained 0·0575 gram when evaporated with ammonia, = 17·37 per cent. NH_3.

Calculated for $C_7H_3NO_4(NH_4)_2$, 16·91 per cent.

Calcium salt.—Prepared by adding calcium chloride to a

hot solution of the ammonium salt. On cooling, small plates crystallized out. After being dried over sulphuric acid it was analyzed.

Taken .....0·2530 gram.
Loss at 150°...0·0310 gram,=12·25 per cent. H_2O.

The residue, after conversion into $CaCO_3$, weighed 0·1116 gram,=20·11 per cent. Ca in the dried acid. $C_7H_3NO_4Ca\,2H_2O$ contains 14·93 per cent. H_2O; and $C_7H_3NO_4Ca$, 19·51 per cent. Ca.

Silver salt.—The silver salt comes down on addition of silver nitrate to a solution of the acid as a flocculent precipitate, closely resembling that of the β-acid, but not nearly so gelatinous or bulky as that of the α-acid. It is very sparingly soluble in water.

I. Prepared by adding silver nitrate to the acid.

Taken... 0·4475 gram.
Ag ..... 0·2465 gram,=55·08 per cent.

II. and III. Prepared by adding silver nitrate to the ammonium salt.

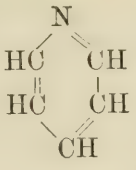
Taken... 0·2967 gram.
Ag ..... 0·1673 gram,=56·38 per cent.
Taken... 0·2305 gram.
Ag ..... 0·1305 gram,=56·61 per cent.

$C_7H_3NO_4Ag_2$ contains 56·69 per cent. of silver.

γ-Dicarbopyridenyl chloride.—The chloride was prepared in the usual manner; it boiled at 265°. On exposure to air it turns bluish-violet, and liquefies. It solidifies with great reluctance at the ordinary temperature; and yet its melting-point is at 88–89°. It appears to be much more easily decomposed by water than the chlorides of the α- and β-acids, for it turns liquid at once on treatment with cold water.

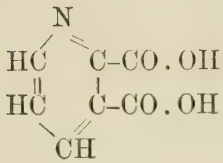
Bitter substance.—The bitter substance already mentioned remained in the mother-liquor of the β- and γ-dicarbopyridenic acids. It was partially separated by adding lead or silver nitrates to a *neutral* solution; for both these salts are soluble in acid. The lead salt, when decomposed by sulphuretted hydrogen, gave a solution which was evaporated to a syrup. The bitter substance does not crystallize from water or alcohol; it is extremely soluble in both these solvents, but less easily soluble in ether. It is an acid; its barium salt is a syrup, which dries up to a vitreous mass over sulphuric acid. The acid, as well as its salts, give off the usual smell of pyridine when heated, and also give a red coloration with ferrous sulphate. It was unfortunately lost, after it had been separated in a nearly pure state.

Theoretical considerations.—If the formula proposed for pyridine by Alder Wright, be accepted as correct, six isomeric acids of the formula $C_5H_3N(CO.OH)_2$ are theoretically possible. They are as follows :—



Pyridine.

1.



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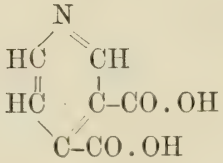

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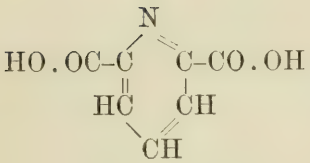

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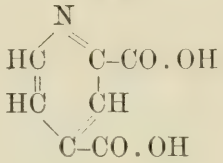

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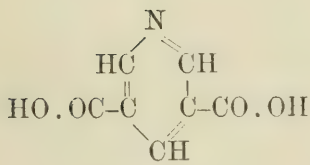

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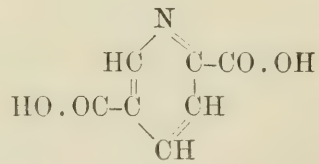


Even if structural formulæ be discarded, it is evident from the analogy between pyridine and chinoline, and benzol and naphthalene, that a number of isomerides are to be expected when two external groups replace two atoms of hydrogen in pyridine. In the case of benzol, three isomerides are known under such conditions; and it is to be expected that pyridine, a more complicated substance than benzol, owing to its containing an atom of nitrogen, should yield more than three isomeric bodies.

Now, that α-, β-, and γ-dicarbopyridenic acids are isomeric, and not identical, is rendered highly probable by the following considerations :—the β-acid is much less soluble in water than the other two modifications ; as a rule its salts are much less soluble ; its crystalline form is different. Copper sulphate produces no precipitate with the α-acid, whereas the other two

give whitish blue precipitates. With ferric chloride, the β- and γ-acids give a white precipitate, which is changed to ferric hydrate on addition of ammonia; the α-acid gives no precipitate with ferric chloride, even after addition of excess of ammonia, showing that ferric hydrate is soluble in the ammonium salt of the acid. The methyl-ether of the α-acid forms thick isolated crystals; that of the β-acid consists of deliquescent needles. The chloride of the α-acid boils at 284°, that of the β-acid at 269–270°, and the chloride of the γ-acid at 265°. The melting-points of the chlorides are respectively:—α, 60·5–61°; β, 49°; γ, 88–89°.

I may anticipate that the acid obtained by oxidation of quinine, and since found to result from the four chief alkaloids of the cinchona-bark, and described by Mr. James Dobbie and myself in the 'Journal of the Chemical Society' for April of the current year, has the formula $C_7H_5NO_4$; but, from its crystalline form and properties, it appears to be distinct from any one of the acids described in this memoir. This subject, however, demands a more complete examination than we have as yet bestowed on it. It is my intention, should it prove different, to publish a table giving the characters of the isomeric acids.

It is also possible that the bitter substance, which, like these acids, gave off pyridine when heated, is also an isomeride; but of this I have no proof.

Dipyridine.—In purifying some oils, afterwards to be described, I obtained a considerable quantity of dipyridine, $C_{10}H_{10}N_2$, and purified it by crystallization from hot water. It presented the appearance of long hair-like white needles. As its vapour-density had only been once determined, it was deemed of sufficient interest to redetermine it, by means of Meyer's apparatus.

The amount taken was 0·0247 gram. The formula by which it was calculated is

$$D=\frac{0{\cdot}0247\times 1543500\times 14{\cdot}47}{\left\{743{\cdot}5+\left(36\times\frac{2}{3}\right)\right\}\left\{\left(\frac{213{\cdot}7}{9{\cdot}608}\right)1{\cdot}01-\left(\frac{119{\cdot}8}{9{\cdot}158}\right)\right\}}=76{\cdot}63$$

Reference is made to the previous example for the meaning of the numbers. The theoretical density of dipyridine vapour compared with hydrogen is 79.

Isodipyridine.—Some test tubes full of oil, left by the late Professor Anderson, and labelled "Bases forced over from the residue on distillation of dipyridine," were submitted to examination. When heated at the ordinary atmospheric pressure, these bases boiled at a temperature above that at which

mercury boils; they were therefore fractionated in a vacuum produced by a Sprengel's air-pump. After numerous distillations, a fraction was separated, which boiled *in vacuo* at 145–155°, and in air at 295–305°. Two determinations of the vapour-density of this oil gave the following numbers:

I. 81·55; II. 82·80.

These numbers agree sufficiently well with the vapour-density of dipyridine, 79.

On analysis it gave the following results :—

I. Taken, 0·3935 gram.

CO_2 ..... 1·1272 gram, = 78·10 per cent. C.
H_2O ..... 0·2362 gram, = 6·67 per cent. H.

II. Taken, 0·3003 gram.

CO_2 ..... 0·8750 gram, = 79·06 per cent. C.
H_2O ..... 0·1778 gram, = 6·57 per cent. H.

III. Taken, 0·2830 gram.

CO_2 ..... 0·8181 gram, = 78·84 per cent. C.
H_4O ..... 0·1717 gram, = 6·74 per cent. H.

Dipyridine, $C_{10}H_{10}N_2$ contains 75·94 per cent. C.
6·38 per cent. H.

Both carbon and hydrogen in every case are too high; and I have been unable to conjecture any reason for the excess. I am inclined to believe, from the vapour-density, as well as from analyses of some compounds of the base, that it really has the formula of dipyridine.

Isodipyridine is a yellow viscid oil, of 1·08 specific gravity. It boils at 145–155° in a vacuum, and at 295–305° at the ordinary pressure. It does not solidify on exposure to the cold produced by a mixture of snow and salt, nor is crystallization induced by addition of crystals of dipyridine. It dissolves very sparingly in water, and communicates to it its characteristic heavy basic smell; it mixes in all proportions with alcohol and ether.

Isodipyridine hydrochloride.—When a solution of isodipyridine is evaporated to dryness with hydrochloric acid, redissolved in alcohol, and allowed to remain over sulphuric acid, small, hard, white crystals are deposited. With platinic chloride, the solution of this salt gives a yellow crystalline precipitate of the *platinichloride*. A determination of platinum was made with the following results :—

Taken, 0·3903 gram. Loss at 110°, 0·0085 gram, = 2·43 per cent.

Pt. ..... 0·1298 gram, = 34·34 per cent.

$C_{10}H_{10}N_2 . 2HCl . PtCl_4$ contains 34·51 per cent. of platinum, and with one molecule of water, 3·07 per cent. H_2O.

Isodipyridine methyl-iodide.—When isodipyridine is mixed with excess of methyl iodide, the mixture grows warm, and a brown oil deposits in drops on the side of the tube. After the reaction is over, addition of anhydrous alcohol changes this brown oil into a brilliant red powder. The powder was analyzed.

Taken	.....	0·2015 gram.
Ag I	.....	0·1985 gram } = 58·14 per cent. of iodine.
Ag	.....	0·0085 gram }

$C_{10}H_{10}N_2 . 2CH_3I$ contains 57·99 per cent. of iodine.

This red powder is insoluble in absolute alcohol and ether. In aqueous alcohol it dissolves slightly with a yellow colour; and with water it forms a nearly colourless solution.

The *methyl-chloride* is a yellow syrup, which gives a yellow precipitate with platinic chloride, of the formula

$$C_{10}H_{10}N_2 . 2CH_3Cl . PtCl_4.$$

It decomposes partially at 100°.

Taken	.....	0·3426 gram.
Pt	.....	0·1126 gram, = 32·86 per cent.

Calculated for $C_{10}H_{10}N_2 . 2CH_3Cl . PtCl_4$, 32·94 per cent. The small amount of isodipyridine at my disposal made it impossible to prepare more salts. In distilling this base at the atmospheric pressure, the last few drops fell on a red-hot surface; and on opening the bulb a strong smell of pyridine was perceived. This base therefore is apparently converted into pyridine by exposure to a high temperature.

The methyl-iodide of dipyridine, prepared from pure crystallized base, is also a red powder; I failed entirely to observe the white needles described by Dr. Anderson in his account of the base.

Dipicoline, $C_{12}H_{14}N_2$.—Dipicoline was prepared by Dr. Anderson by allowing picoline to remain in contact with metallic sodium. A bottle, set aside by him in 1874, contained a black, tarry mass; on exposure to moist air, it gradually became yellow, while the sodium unattacked by the picoline oxidized. After the whole had become yellow, water was added, and the oil, which sank to the bottom, was well washed to remove sodium hydrate. After an attempt to purify it by distillation with water-vapour (which did not succeed, owing to the slight volatility of the base), the oil was dried, and distilled alone. A large quantity of picoline came over at 134°; the temperature then rose rapidly to 280°. A small fraction

distilled between 280° and 310°; but by far the largest portion distilled between 310° and 320°. There was very little remaining; so that the fraction last mentioned constituted the bulk of the oil. After two more distillations, the portion boiling at 310–320° being collected separately, the base was considered sufficiently pure for investigation.

Its vapour-density was determined twice:

$$\text{I. } D = \frac{0{\cdot}0420 \text{ gram} \times 1543500 \times 14{\cdot}47}{760 + \left(42 \times \frac{2}{3}\right)\left\{\left(\frac{221{\cdot}3 \text{ grams}}{9{\cdot}608}\right)1{\cdot}01 - \left(\frac{96{\cdot}1 \text{ grams}}{9{\cdot}158}\right)\right\}} = 95{\cdot}30.$$

$$\text{II. } D = \frac{0{\cdot}0411 \text{ gram} \times 1543500 \times 14{\cdot}47}{752 + \left(45 \times \frac{2}{3}\right)\left\{\left(\frac{253{\cdot}9 \text{ grams}}{9{\cdot}608}\right)1{\cdot}01 - \left(\frac{134{\cdot}7 \text{ grams}}{9{\cdot}158}\right)\right\}} = 97{\cdot}99.$$

The theoretical vapour-density of dipicoline is 93.

Analysis of dipicoline dried over sodium and distilled :—

Taken	.....	0·2932 gram.
CO_2	.....	0·8313 gram, = 77·32 per cent. C.
H_2O	.....	0·1886 gram, = 7·14 per cent. H.

$C_{12}H_{14}N_2$ contains 77·41 per cent. of carbon, and 7·52 per cent. of hydrogen.

Dipicoline, $C_{12}H_{14}N_2$, is a yellow oil, closely resembling dipyridine in appearance. Its smell is similar, but slightly different. It mixes in all proportions with alcohol and ether, and is sparingly soluble in water, to which it communicates its taste and smell. Its specific gravity is 1·12, rather more than that of dipyridine. It boils at 310–320° at the ordinary pressure. It unites with acids to form salts which have little tendency to crystallize.

The *hydrochloride*, after standing in a syrupy condition over sulphuric acid for some days, showed a tendency to crystallize at the edge. The *platinichloride* has the formula

$$C_{12}H_{14}N_2 . 2HCl . PtCl_4.$$

Analyses :—

I.	Taken	.....	0·5117 gram.
	Pt	.....	0·1692 gram, = 33·06 per cent.
II.	Taken	.....	0·5069 gram.
	Pt	.....	0·1684 gram, = 33·22 per cent.

The platinichloride contains 33·73 per cent. of platinum. Dipicoline unites with the iodides of the alcohol-radicals; of these compounds, the methyl-iodide was prepared.

Dipicoline methyl-iodide, $C_{12}H_{14}N_2 . 2CH_3I$.—Obtained by the action of excess of methyl-iodide on dipicoline, at the ordinary temperature. It forms a reddish-brown oil, which solidifies after some time. On treatment with anhydrous alcohol, it changed to a bright yellow powder, almost insoluble in alcohol and in ether, but readily soluble in water, giving an almost colourless solution. It was dried and analyzed.

Taken	.....	0·2764 gram
Ag I	.....	0·2638 gram } =54·29 per cent. I.
Ag	.....	0·0085 gram }

$C_{12}H_{14}N_2 . 2CH_3I$ contains 54·24 per cent. of iodine.

The *methyl-hydrate*, prepared by addition of silver oxide and water to the methyl-iodide formed a colourless liquid, which turned brown and evolved a peculiar smell on evaporation.

The *methyl-chloride*, obtained by addition of hydrochloric acid to the methyl-hydrate, is a white crystalline salt. The *platinichloride* is a buff-coloured precipitate, soluble in hot water, and crystallizing out in yellow crystals when the solution cools. It was analyzed.

Taken	.....	0·1120 gram.
Pt	.....	0·0345 gram, =30·80 per cent.

$C_{12}H_{14}N_2 . 2CH_3Cl . PtCl_4$ contains 31·47 per cent. of platinum.

A strong solution of iodine in alcohol was added to a hot solution of the methyl-iodide in aqueous alcohol; brown scales with coppery lustre separated out on cooling. This compound was analyzed.

Taken	.....	0·4125 gram,
Ag I	.....	0·6245 gram, =81·81 per cent. of I.

By titration with sodium thiosulphate, this compound was found to contain 61·25 per cent. of free iodine.

$$C_{12}H_{14}N_2 . 2CH_3I . I_6,$$

the *hexiodide of dipicoline methyl-iodide*, contains 82·46 per cent. of total iodine, and 61·85 per cent. of additive iodine. This compound is insoluble in water, but dissolves sparingly in alcohol with a deep yellow colour. It is insoluble in carbon disulphide.

An aqueous solution of dipicoline, on addition of bromine water, gave a very bulky buff-coloured precipitate. It was dried over sulphuric acid and analyzed.

Taken	.....	0·4108 gram.
Ag Br	.....	0·5161 gram, =53·46 per cent. of Br.

$C_{12}H_{11}Br_3N_2$ contains 56·73 per cent. of bromine.

$C_{12}H_{13}BrN_2.2HBr$ contains 55·70 per cent. of bromine. I am disposed to accept the latter formula as the correct one; for one atom of bromine introduced into the molecule of dipicoline would probably not destroy its basic property.

Oxidation of Dipicoline.—Five grams of dipicoline were oxidized in the usual manner with potassium permanganate. The acids when separated from their lead salts, consisted:—(1) of an acid, almost totally insoluble in water, alcohol, and ether, and separating in amorphous flocks on evaporation of the solution containing other acids, in which it dissolves sparingly; it gives off a smell of the polymerized bases when heated on platinum foil; and (2) a mixture of acids (certainly containing no α-dicarbopyridenic acid), of which the quantity at my disposal was too small to admit of fractional crystallization. These acids also evolved a smell resembling that of dipyridine, when heated. The lead and silver salts of these acids are white insoluble precipitates. It is probable, from the fact of these acids evolving the smell of dipyridine when heated, that dipicoline, when oxidized, yields a polymeride of dicarbopyridenic acid.

General Conclusions.

1. Bases of the series $C_nH_{2n-5}N$ are tertiary bases; they are not attacked by nitrous acid; nor do they unite with more than one molecule of a halogen compound of an alcohol radical.

2. They are unsaturated compounds, but have no great tendency to form addition-compounds. The addition-compounds are divisible into three classes:—(a) compounds in which the base combines directly with an acid to form a salt; (b) compounds in which the base unites with two atoms of a halogen, *e. g.* picoline chloriodide, $C_6H_7N.ClI$; and (c) those in which a salt combines with two atoms of a halogen, in the pyridine series, as, for example, the diniodide of picoline methyl-iodide, $C_6H_7N.CH_3I.I_2$, and with six atoms of a halogen, as in the case of the hexiodide of dipicoline methyl-iodide, $C_{12}H_{14}N.(CH_3I)_2I_6$.

3. Like the paraffins, they are not attacked by acid oxidizing agents in the cold. They differ from paraffins by withstanding such action even at a high temperature; and this is probably owing to the increased stability given to the molecule by nitrogen, which renders them basic, and imparts to them the property of forming salts. The heat of formation of these bases is doubtless very high; and when a still greater amount of heat is evolved by their combination with acids, the sum of

heat-units evolved by the union of the carbon, hydrogen, and nitrogen, plus that evolved by the formation of their salts, is probably greater than that evolved by their oxidation.

4. It is consequently only in alkaline solution that they can be oxidized. In this case, the amount of heat evolved by the acid formed by oxidation uniting with the alkali increases the sum of thermal units evolved by their oxidation to a number larger than that evolved during the formation of the base. The amount of heat evolved, moreover, is not increased by that arising from a union of the base with an acid.

5. At least three isomerides are formed by replacement of two atoms of hydrogen in pyridine by carboxyl, COOH. From analogy with benzol, and consideration of the greater complexity of the molecule of pyridine, it is highly probable that more than three exist. These acids are produced by oxidizing picoline and its higher homologues, and probably also by oxidizing pyridine. This behaviour is analogous to that of benzol; for benzol yields acids containing more than six atoms of carbon when oxidized. From picoline, C_6H_7N, it was to be expected that monocarbopyridenic acid, $C_5H_4N.COOH$, should be produced; this acid has been obtained only by the oxidation of nicotine; α-dicarbopyridenic acid is the only one produced in large quantity by the oxidation of picoline. When lutidine, C_7H_9N, is oxidized, a mixture of at least three isomeric acids of the formula $C_5H_3N(CO.OH)_2$ is formed. This would imply the existence of as many different isomeric lutidines: and indeed it is highly probable that such isomerides exist; for great difficulty is experienced in separating isomeric liquids, especially (as in this case) when the presumption is that their boiling-points are almost identical.

6. These isomerides may be represented in graphic formulæ as on page 27. A closed chain appears best suited to express the behaviour of the bases and the isomeric compounds derived from them. I failed in several attempts to convert the dicarbopyridenic acid into its alcohol (from which I had hoped to obtain a base), owing to the instability of the acid at a high temperature and the small yield of aldehyde when the acid was distilled with calcium formate. I had hoped to achieve the formation of the base by the following stages:—
$C_5H_3N(CO.OH)$; $C_5H_3N(CHO)_2$; $C_5H_3N(CH_2OH)_2$; $C_5H_3N.(CH_2Cl)_2$; and $C_5H_3N(CH_3)_2$.

From similar reasons, an attempt to prepare lutidine, C_7H_9N, by distilling the methyl ether of the α-acid proved abortive; in every case, pyridine was formed. In spite of these failures, it appears to me probable that picoline is *methyl-pyridine*, lutidine *dimethyl-pyridine*, etc.,—the presence of

nitrogen, as conjectured in (3), giving stability to the molecule, and preventing the oxidation of the methyl-groups to carboxyl.

7. On treatment with sodium, these bases are polymerized, no hydrogen being evolved by the action of the sodium on the base. An addition-product is consequently formed, probably by two atoms of sodium being taken up by each molecule of base ; on coming into contact with a fresh molecule of base, the sodium leaves the first molecule, which has thereby its affinities free for union with another similarly placed. I am very doubtful if any *compound* of picoline and sodium is formed. Certainly sodium does not combine with dipicoline ; for that base can be freed from water by heating it to a high temperature with metallic sodium.

8. In conclusion, I would remark the analogy between the furfurol and pyridine groups. That they are closely related appears very probable. An attempt to effect the conversion of furfurol, $C_5H_4O_2$, into pyridine through the following series of reactions, failed owing to the instability of furfuryl chloride.

$C_5H_4O_2$,	$C_5H_6O_2$,	C_5H_5OCl,	$C_5H_5ONH_2$,	C_5H_5N.
Furfurol.	Furfuryl alcohol.	Furfuryl chloride (unknown).	Furfuryl amine (unknown).	Pyridine.

The furfurol group, from its unstable nature, is probably analogous to the higher homologues of acetylene, and is best represented by an open chain ; whereas the pyridine group, from its stability, and from the number of isomeric derivatives obtained from it, is, like benzol, best pictured by a closed chain.

In closing this research, I have to express my deep indebtedness to Professor Ferguson for placing at my disposal the material which belonged to the late Professor Anderson.

IV. *On Salt Solutions and Attached Water.*

By Frederick Guthrie*.

[Continued from vol. ii. p. 225.]

VI.

Further Examples of Cryohydrates and Cryogens.

§ 167. *Hydrate of Barium.*—On cooling a saturated solution of this salt, the well-known recognizable crystals are continually deposited until the temperature reaches

* Communicated by the Physical Society.

$-0°{\cdot}5$. At this temperature the cryohydrate is formed, the temperature remaining constant until solidification is complete. The barium was estimated by adding carbonate of ammonium and gently igniting.

grams.	$BaO\,CO_2$.	BaO.	BaO per cent.
22·1204	gave 0·4271	or 0·331707	or 1·4995
19·6436	„ 0·3790	„ 0·29435	„ 1·4984
14·7921	„ 0·2924	„ 0·22709	„ 1·5352

The above analyses are of three separate liquid residues after the separation of two crops of cryohydrate in each case.

The water-worth calculated on the whole three of these results is 565.

As a cryogen the same temperature ($-0°{\cdot}5$) was obtained.

§ 168. *Hydrate of Strontium.*—On lowering the temperature of a saturated solution of this hydrate, crystals of the same separated until the temperature $-0°{\cdot}1$ was reached; the cryohydrate then formed.

grams.	$SrO\,CO_2$.	SrO.	SrO per cent.
17·2620	gave 0·0944	or 0·5468	or 0·3838
13·5124	„ 0·0774	„ 0·5728	„ 0·4020

The analyses were of different liquid residues after two crops of cryohydrate had been separated from each. The water-worth derived from the mean of these two analyses appears to be 1463. As cryogens, $SrO\,H_2O$ or SrO when mixed with ice did not lower the temperature below 0°. A very slight fall to $-0°{\cdot}09$ was obtained on slaking SrO with water and allowing it to assume the normal temperature before mixing with ice. Such a fall, however, can scarcely be recognized.

§ 169. *Hydrate of Calcium.*—This body, as is well known, presents the very interesting peculiarity of being more soluble in cold than in hot water. On further cooling the solution saturated at a certain temperature above 0°, one gets an imperfectly saturated solution; and on still further cooling, ice appears. But if the solution be kept saturated during cooling, which can be well effected by continued stirring with some hydrate precipitated by rise of temperature in another quantity, the cryohydrate appears at $-0°{\cdot}15$.

grams.	$CaO\,CO_2$.	CaO.	CaO per cent.
22·1094	gave 0·1030	or 0·057630	or 0·26
23·3498	„ 0·1222	„ 0·068432	„ 0·29

The analyses were of different liquid residues after two crops of cryohydrate had separated: the mean of these two results corresponds to the relation

$$CaO + 1116\,H_2O.$$

Employed as a cryogen, $-0°{\cdot}18$ was the lowest temperature reached. The only data I can find of the solubility of lime at different temperatures are those of Dalton; from whose experiments it appeared that lime-water saturated at

100° C.	contains	0·1050	per cent.	of $CaO\,H_2O$
54·4	„	0·1371	„	„
15·5	„	0·1712	„	„

From my results it appears that, the mean CaO being 0·275 per cent. (the mean of two analyses of the cryohydrate at $-0°{\cdot}15$ gave 0·3634 per cent. of $CaO\,H_2O$), lime is $3\frac{1}{2}$ times more soluble in water at $-0°{\cdot}15$ C. than at 100 C.

With regard to these water-worths,

	W.W.
Hydrate of barium	565
„ strontium . . .	1463
„ calcium	1116

it is not only noteworthy, but imperatively demands notice, that here calcium lies between barium and strontium. They who are engaged with the spectra of these metals will scarcely need a further hint.

The melted cryohydrates of $SrO\,H_2O$ and $CaO\,H_2O$, which had been solidified in an ice and NaCl freezing-mixture, did not exhibit the characteristic opacity of cryohydrates. They became milky in a carbonic acid and ether cryogen, but did not seem even then to assume the characteristic opacity. A trace of this true opacity appeared with $BaO\,H_2O$.

§ 170. *Hydrate of Potassium.*—A concentrated solution, namely a solution resulting from the deliquescence of the solid hydrate in moist air free from carbonic acid, did not solidify on being subjected to a cryogen of CO_2 and ether: it, however, became syrupy. A dilute solution (indefinite) solidified wholly within the range of an ice-and-NaCl cryogen. The solid was transparent and resembled ice; it sank in water at 0° C.

As a cryogen, caustic potash gave a temperature of $-19°{\cdot}2$ C. But, as will appear subsequently (and this is an important point), a salt which evolves heat on mixing with water makes a more powerful cryogen with ice if previously cooled; not so those which do not.

§ 171. *Permanganate of Potassium.*—On cooling a saturated solution of this salt, crystals of the permanganate separated out down to 0° C. The cryohydrate was formed at $-0°{\cdot}57$; and the temperature then ceased to sink. The estimation of the water was by evaporation at 100°. The samples are from

different preparations. In each case several crops of the solid cryohydrate had been removed.

grams.	Anhydrous salt.	Per cent.
20·4624	0·5874	2·871
19·6134	0·5910	3·013
19·7115	0·5249	2·663
19·7476	0·5318	2·693
12·6320	0·3718	2·943

The mean of these rather discordant results shows a water-worth of 608·3 ($K_2 MnO_8$).

As a cryogen, permanganate of potash gives a temperature of $-0°·52$.

§ 172. *Acetate of Lead.*—The cryohydrate of acetate of lead forms at $-1°·4$ C. After the separation of several crops, in each case the residual liquids, after weighing, were repeatedly evaporated with an excess of nitric acid and gently heated. It is found that the acetate is thereby completely converted into the nitrate.

grams.	Nitrate.		Per cent. of acetate.
14·8195	gave 2·7906	=	18·4
6·9029	„ 1·2401	=	17·5
7·4904	„ 1·3700	=	18·0

The water-worth, calculated on the mean of these three results, is 82·3. The temperature of the acetate-of-lead cryogen is $-1°·7$.

§ 173. *Sulphate of Zinc and Potassium.*—The cryohydrate forms at $-1°·25$. The analyses are, as before, of different residual liquids. The liquid cryohydrate was evaporated to dryness at 100° and heated to incipient fusion.

grams.	$ZnSO_4 + K_2SO_4$.	Per cent.
9·1020	yielded 0·7274	or 7·992
9·0934	„ 0·7739	„ 8·510

The mean of these results gives the water-worth 167·4. As a cryogen, the same double salt gives $-1°·01$. The temperature of solidification of this cryohydrate and the percentage of salt it contains are approximately those of $K_2 SO_4$, its least-soluble constituent (see §§ 22, 71).

	Temperature of cryohydrate.	Per cent. of salt.
$K_2 SO_4$	$-1°·5$	7·8
$ZnSO_4$	$-7°$	30·84
$K_2 SO_4 + ZnSO_4$	$-1°·25$	8·25

Comparing this fact with those developed in §§ 109–120,

where mixtures of salts were employed, we find that while with such mixtures the more powerful cryogen governs the temperature, here it is the less powerful cryogen which does so.

§ 174. *Ferrocyanide of Potassium*—The cryohydrate of this salt forms at $-1°{\cdot}7$. Two analyses of the same residual liquid were made by evaporating at 100°, and subsequent stirring with free exposure to the air at the same temperature.

grams.	K_4FeCy_6.	Per cent.
6·6510	gave 0·7987	or 12
6·5080	„ 0·7665	„ 11·8

The mean indicates the formula

$$K_4FeCy_6 + 151{\cdot}6\,H_2O.$$

The temperature of the cryogen is $-1°{\cdot}61$. If this body had been the first to be experimented on in the direction which has led to the discovery of the cryohydrates, it is possible that the whole class might have escaped notice, because, when the cryohydrate is allowed to melt, a considerable quantity of yellow salt (terhydrate or subcryohydrate) falls down unless the liquid is kept well agitated. Though not peculiar to this salt, it is perhaps more marked with it than with any other. To be viewed almost as a corollary to the above fact is the following one concerning the same salt. When a solution of the strength of the cryohydrate is cooled, there may be a simultaneous formation of ice and some hydrated or anhydrous ferrocyanide: the one floats; the other sinks. As before pointed out in other cases, this independent separation of the constituents of the cryohydrate is entirely prevented by placing in the solution a fragment of previously formed cryohydrate.

§ 175. *Ferricyanide of Potassium.*—At a temperature of $-3°{\cdot}9$ a saturated solution of this salt began to give up its cryohydrate, having previously given up a less hydrated salt. Though several determinations of the amount of salt in the residual liquid were made both by evaporation over a water-bath and drying *in vacuo* over sulphuric acid, the results differed from one another by several per cent. The lowest was 19·8 per cent., the highest 24 per cent. The temperature of the cryogen is $-3°{\cdot}9$ C. The ambiguity in the amount of water in the cryohydrate finds its counterpart in the similar ambiguity in the water of crystallization of the ordinary salt.

§ 176. *Nitrate of Urea.*—The cryohydrate first appears at $-4°$, on cooling a saturated solution. The residual liquids from two preparations, after about the same quantity of cryohydrate had separated, were very gently evaporated on a water-

bath only just to complete dryness. If the substance be further heated, minute iridescent crystals begin to appear at that part of the residue where the basin is in contact with the bath. As soon as these began to appear the further heating was stopped.

grams.		Nitrate.		Per cent.
6·6889	gave	0·5693	or	8·64
6·7616	„	0·5844	„	8·51

The water-worth is accordingly 72·83. As a cryogen the temperature of $-4°{\cdot}5$ is obtained.

§ 177. *Oxalate of Potassium.*—The temperature at which the cryohydrate formed was found to be $-6°\,3$. On evaporating 3·2862 grams in a platinum capsule, igniting, and quickly cooling on a metal slab, 0·5790 of the carbonate was obtained (=17·62 per cent.).

In another analysis, 3·1500 grams, on evaporation to dryness on a water-bath, gave 0·6684 oxalate (21·22 per cent.). This gave on ignition 0·5531 of carbonate (17·56 per cent.), which by treatment with hydrochloric acid gave 0·5958 of chloride of potassium (18·88 per cent.). The percentage of carbonate agrees closely with that of the first analysis. The percentage of chloride differs only by 0·07 from that required on the conversion of 17·56 of carbonate. Hence it would seem that with due care oxalate of potassium may safely be estimated as carbonate, notwithstanding the hygroscopic character of the latter salt. It also appears (contrary to statements sometimes made) that oxalate of potassium does not retain any of its water of crystallization when its solution is evaporated to dryness at 100° C.; for the 17·56 per cent. of carbonate obtained is equivalent to 21·12 per cent. of oxalate, which only differs by 0·1 per cent. from 21·22, which was found. The water-worth (deduced from the chloride of potassium) is 17·3.

The temperature of the cryogen is $-6°{\cdot}2$.

§ 178. *Fluoride of Sodium.*—This salt, like several other salts of sodium, is troublesome when attacked from the side of saturated solutions, because there appears to be a subcryohydrate. This salt also exhibits the very rare property of being almost equally soluble at all temperatures between 100° and $-3°{\cdot}9$. On cooling a hot saturated solution to $-2°$, nothing separates. Between $-2°$ and $-4°$ an ice-like body is formed. The true cryohydrate appears to be formed at $-5°{\cdot}63$; for this temperature is preserved till the whole is solid and opaque. Fluoride of sodium, as a cryogen, gives $-3°{\cdot}2$. The cryohydrate has not been further examined.

§ 179. *Cyanide of Mercury.*—A concentrated solution gave

up the anhydrous salt until $-0°{\cdot}45$ was reached; then the cryohydrate was formed. The amount of salt was determined by evaporation over sulphuric acid *in vacuo*.

grams.	Cyanide.
3·8835	gave 0·2892 or 7·45 per cent.
5·0238	„ 0·3738 or 7·44 „

This percentage exhibits the water-worth of 174. The same salt as a cryogen reaches the temperature $-0°{\cdot}6$.

§ 180. *Acetate of Zinc.*—A boiling saturated solution was allowed to cool to the atmospheric temperature ($+15°$). The residual liquid was drained from the separated crystals and introduced, together with a few crystals, into a stoppered bottle. After keeping in ice for nine hours with frequent agitation, all sign of further crystallization ceased. The zinc was estimated by precipitation with carbonate of sodium. It was thus found that the strength at 0° was 23 per cent. This solution, when artificially cooled, yields fern-like crystals at $-5°{\cdot}9$; these gradually became opaque, the opacity forming in a frond-like manner; the whole became dry at the above temperature. An analysis of the residual liquid, after two crops of cryohydrate, showed that the cryohydrate had sensibly the same composition as the solution saturated at 0° C.; for

grms.	Oxide.
5·9980	gave 0·6103 or 23 per cent. anhydrous acetate.

§ 181. *Hyposulphite of Soda.*—The following are the temperatures at which solidification begins in solutions of varying strengths of this salt:—

TABLE XXXIX.

$Na_2S_2O_3$. by weight.	Temperature, Centigrade.	Body found.
per cent.	°	
1	− 0·1	Ice.
2	− 0·4	„
3	− 0·65	„
5	− 1·2	„
6	− 1·5	Appearance of ice, but sinks in water at 0°. Perhaps due to interstitial solution. More probably a suberyohydrate.
10	− 2·5	
15	− 3·9	
20	− 5·45	
30	− 9·5 to −11·0	
30	−11	Cryohydrate.
33·5·5	0·0 (Kremers).	Ordinary hydrate.
41	+20 (Kremers).	

A 30-per-cent. solution does not always give up a solid at $-9°{\cdot}5$. Sometimes the temperature sinks to $-12°{\cdot}4$; then

the true cryohydrate is formed, and the temperature rises to -11° and remains constant till all is solid.

The solubilities at 0° and 20° are those given by Kremers. These and the determination for -11°, when plotted in the usual way, are found to lie on the same straight line. H. Schiff found at $19^\circ{\cdot}5$ a saturated solution to contain 63·5 per cent. five-hydrated, or 45·8 per per cent. anhydrous salt. As a cryogen, the temperature -10° was reached.

§ 182. *Citric Acid.*—This body presented many difficulties; but as these difficulties occur again with most organic acids of high molecular weight, a special study was made of it. It is peculiarly liable in aqueous solution to supersaturation of the most persistent kind, especially when the solution is at a low temperature. At temperatures and under conditions which are capable of evolving the cryohydrate, the solution assumes sometimes an almost colloidal form, and shows no signs of eliminating solids unless other means besides mere lowering of temperature are employed.

From solutions ranging from 10 to 40 per cent. of anhydrous acid, ice is liberated; and this continues to 42·26 per cent., from which solution a cryohydrate separates at $-9^\circ{\cdot}2$. The following is a somewhat detailed account of the behaviour of solutions containing a greater percentage of acid than 40.

Two grams of a solution gave 0·8525 of citric acid, or 42·28 per cent. This gives a solid at $-9^\circ{\cdot}2$, which at first floats on the residual liquid. The solid consists of massive white agglomerated crystals. The crystals are hexagonal, but present rhomboidal elements, causing the edge of each crystal to be deeply and regularly serrate. When they melt, the rhomboidal crystals are themselves resolved into long slender prisms. A large quantity of such a solution retained its composition when nine tenths of it had been removed by solidification, nevertheless, if such a solution be kept perfectly still for many hours at -9°, a few ice spicula may be formed.

Other solutions, containing respectively 45, 45·9, 50, 50·7, and 51·5 per cent. of the anhydrous acid, were examined. It is only this latter which yields, on cooling, distinct quantities of the original salt: this it does at -6°, but only if a particle of the original salt be introduced and by diligent stirring. When undisturbed, this 51·5 solution may be cooled to $-19^\circ{\cdot}5$ without any solidification. So prone is this acid to exhibit supersaturation, that solutions both weaker and stronger than the 42·62 may be enriched on partial solidification. Thus a 50-per-cent. solution, though already stronger than the cryohydrate, may become still stronger by the separation of ice at -17°. There is therefore a large region of double supersa-

turation; the ice-curve crosses the acid or subcryohydrate-curve, both continuing their courses for an exceptionally long distance.

For the solubility at 0° C., a solution saturated above 0° C. was kept at 0° surrounded by ice and placed in an ice-safe for three days.

Solution.	Anhydrous acid.	Per cent.	Mean.
2·0962 gave	1·0755	or 51·30	=51·285.
2·1288 „	1·0914	„ 51·27	

Crystallized citric acid when added to water has a considerable cooling effect. Thus

110·9 grams of crystallized acid at 20°·5 C., added to
89 „ water at . . . 16°·5
lowered the temperature to . 2°·5;

while

51·5 grams of anhydrous acid cooled to 0° C. and added to
48·5 „ water cooled to . . 0°
gave a temperature of . . −6°.

The chief results may be summarized in the following Table:—

TABLE XL.

Citric acid (anhydrous).

Anhydrous acid, per cent.	Temperature at which solidification begins.	Body formed.
10	− 1°·1	Ice.
20	− 2·8	„
30	− 5	„
40	− 8·5	„
42·62	− 9·2	Cryohydrate.
45	−11·3	At these temperatures ordinary hydrate, subcryohydrate, or even ice may be formed.
45·93	−11·7	
47·06	−12·2	
50·7	−13·7	
51·5	−15	

As a cryogen, the lowest temperature attainable is −9°·3; and this confirms the composition of the cryohydrate which had been deduced synthetically. Neither cooling the acid to 0° C. nor cooling the two separately to −9° C. had any effect upon the temperature; but, of course, the more nearly the initial temperature is to the final one the less is the quantity of liquid formed.

Miscellaneous Notes.

§ 183. The following notes of salts which have not yet been fully examined may be useful.

Cyanide of Potassium, as a cryogen, gives a temperature of −21°·1. The cryohydrate forms at −33°, with a carbonic-acid and-ether cryogen. Compare § 170.

Oxalate of Sodium forms a cryohydrate at −1°·7 C.

Employed as cryogens, the following temperatures were obtained from the corresponding salts:—

	°
Chloride of cadmium . . .	− 8·3 C.
„ nickel	−10·35
Citrate of sodium	−11·3
Acetate of calcium	−11·8
Chloride of cobalt	−15·35
„ manganese . . .	−28·0

Those of these bodies which evolve heat on mixture with water would, when cooled, depress the temperature more. Thus the chloride of manganese scarcely showed signs of a cryohydrate at −40° C.

Formate of Sodium, as a cryogen, gives −14°·3. A concentrated solution becomes semisolid at −14°, but does not become opaque or completely solid in a salt-ice cryogen (−22°).

Tannic Acid, as a cryogen, gives −1°·5.

Sulphurous Acid gives a cryohydrate at −1°·5.

Boracic Acid, as a cryogen, gives −0°·8. The cryohydrate forms at −0°·7.

Arsenious Acid.—The cryogen stands at −0°·3; the cryohydrate formed at −0°·5. Two samples of the melted and liquid cryohydrate were sealed hermetically. After two or three days it was found that a considerable quantity of a fine white powder had exhibited itself.

[To be continued.]

V. *On the Physical Action of the Microphone.* *By* Prof. Hughes.*

IN the paper read on the 9th of May before the Royal Society, I gave a general outline of the discoveries I had made, the materials used, and the forms of microphone employed in demonstrating important points. I have made a great number of microphones, each for some special purpose, varying in form, mechanical arrangement, and materials. It

* Communicated by the Physical Society, having been read June 8, 1878.

would require too much time to describe even a few of them ; and as I am anxious in this paper to confine myself to general considerations, I will take it for granted that some of the forms of instrument and the results produced are already known.

The problem which the microphone solves is this—To introduce into an electrical circuit an electrical resistance, which resistance shall vary in exact accord with sonorous vibrations so as to produce an undulatory current of electricity from a constant source, whose wave-length, height, and form shall be an exact representation of the sonorous waves. In the microphone we have an electric conducting material susceptible of being influenced by sonorous vibrations ; and thus we have the first step of the problem.

The second step is one of the highest importance : it is essential that the electrical current flowing be thrown into waves of determinate form by the sole action of the sonorous vibrations. I resolved this by the discovery that when an electric conducting matter in a divided state, either in the form of powder, filings, or surfaces, is put under a certain slight pressure, far less than that which would produce cohesion and more than would allow it to be separated by sonorous vibrations, the following state of things occurs. The molecules at these surfaces being in a comparatively free state, although electrically joined, do of themselves so arrange their form, their number in contact, or their pressure (by increased size or orbit of revolution) that the increase and decrease of electrical resistance of the circuit is altered in a very remarkable manner, so much so as to be almost fabulous.

The problem being solved, it is only necessary to observe certain general considerations to produce an endless variety of microphones, each having a special range of resistance.

The tramp of a fly or the cry of an insect requires little range but great sensitiveness ; and two surfaces, therefore, of chosen materials under a very slight pressure, such as the mere weight of a small superposed conductor, suffice ; but it would be unsuitable for a man's voice, as the vibrations would be too powerful, and would, in fact, go so far beyond the legitimate range that interruptions of contact amounting to the well-known " make and break " would be produced.

A man's voice requires four surfaces of pine charcoal, as is described in my paper to the Royal Society, six of willow charcoal, eight of boxwood, and ten of gas-carbon. The effects, however, are far superior with the four of pine than with either the ten of gas-carbon or any other material as yet used. It should be noted that pine wood is the best resonant material we possess ; and it preserves its structure and quality

when converted into the peculiar charcoal I have discovered and described.

It is not only necessary to vary the number of surfaces and materials in accordance with the range and power of the vibrations, but these surfaces and materials must be put under more or less pressure in accordance with the force of the sonorous vibrations. Thus for a man's voice the surfaces must be under a far greater pressure than for the movements of insects. Still the range of useful effect is very great, as the boxes which I have specially arranged for man's voice are still sensitive to the tick of a watch.

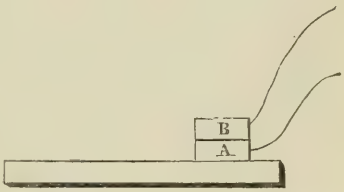
In all cases it should be so arranged that a perfect undulatory current is obtained from the sonorous vibrations of a certain range. Thus, when speaking to a microphone transmitter of human speech, a galvanometer should be placed in the circuit, and, while speaking, the needle should not be deflected, as the waves of + and − electricity are equal, and are too rapid to disturb the needle, which can only indicate a general weakening or strengthening of the current. If the pressure on the materials is not sufficient, we shall have a constant succession of interruptions of contact, and the galvanometer-needle will indicate the fact. If the pressure on the materials is gradually increased, the tones will be loud but wanting in distinctness, the galvanometer indicating interruptions; as the pressure is still increased, the tone becomes clearer, and the galvanometer will be stationary when a maximum of loudness and clearness is attained. If the pressure be further increased, the sounds become weaker though very clear; and as the pressure is still further augmented the sounds die out (as if the speaker were talking and walking away at the same time) until a point is arrived at where there is complete silence.

When the microphone is fixed to a resonant board, the lower contact should be fixed to the board, so that the sonorous vibrations act directly on it. The upper contact, where the pressure is applied, should be as free as possible from the influence of the vibrations, except those directly transmitted to it by the surfaces underneath; it (the upper surface) should have its inertia supplemented by that of a balanced weight. This inertia I find necessary to keep the contact unbroken by powerful vibrations. No spring can supply the required inertia; but an adjustable spring may be used to ensure that the comparatively heavy lever shall duly press on the contacts.

The superposed surfaces in contact may be screwed down by an insulated screw passing through them all, thus doing away with the lever and spring; but this arrangement is far

more difficult to adjust, and the expansion by heat of the screw causes a varying pressure. It is exceedingly simple, however, easily made, and illustrates the theoretical conditions better than the balanced lever I have adopted in practice.

In order to study the theoretical considerations, and that with the most simple form of microphone freed from all surrounding mechanisms, let us take a flat piece of charcoal 2 millims. thick and 1 centim. square, and, after making electrical contact by means of a copper wire on the lower surface, glue that to a small resonant board or, better for the purpose of observation, to a block of wood 10 centims. square. Upon this superpose one or more similar blocks of charcoal, the upper surface in communication with a wire, the lowermost surface resting flat, or as nearly so as possible, on the lower block.



The required pressure is put on the upper block ; and while in this state the two may be fastened together with glue at the sides, or, better, by an insulated screw. The pressure can then be removed, as the screw or glue equally preserves the force. Let the lower piece be called A and the upper B: when we subject this board to sonorous vibrations, we cannot imagine an undulatory movement of the actual wave-length in such a mass, that is a length comparable with the real wave-length of the sonorous wave, which may be several feet. Nor can we imagine a wave of any length without admitting that the force must be transmitted from molecule to molecule throughout the entire length: thus any portion of a wave, of which this block represents a fraction, must be in molecular activity. The lower portion of the charcoal A, being part of the block itself, has this molecular action throughout, transmitting it also to the upper block. How is it that the molecular action at the surfaces of A and B should so vary the conductivity or electrical resistance as to throw it into waves in the exact form of the sonorous vibrations? It cannot be because it throws up the upper portion, making an intermittent current, because the upper portion is fastened to the lower, and the galvanometer does not indicate any interruption of current whatever. It cannot be because the molecules arrange themselves in stratified lines, becoming more or less conductive, as then surfaces would not be required—that is, we should not require discontinuity between the blocks A and B; nor would the upper surface be thrown

up if the pressure be removed, as sand is on a vibrating glass. The throwing-up of this upper piece B when pressure is removed proves that a blow, pressure, or upheaval of the lower portion takes place : that this takes place there cannot be any doubt, as the surface, considered alone (having no depth), could not bodily quit its mass. In fact, there must have been a movement to a certain depth ; and I am inclined to believe, from numerous experiments, that the whole block increases and diminishes in size at all points, in the centre as well as the surface, exactly in accordance with the form of the sonorous wave. Confining our attention, however, to points on A and B, how can this increased molecular size or form produce a change in the electrical waves? This may happen in two ways :—*first*, by increased pressure on the upper surface, due to its enlargement ; or, *second*, the molecules themselves, finding a certain resistance opposed to their upward movement, spread themselves, making innumerable fresh points of contact. Thus an undulatory current would appear to be produced by infinite change in the number of fresh contacts. I am inclined to believe that both actions occur : but the latter seems to me the true explanation ; for if the first were alone true, we should have a far greater effect from metal powder, carbon, or some elastic conductor, such as metallized silk, than from gold or other hard unoxidizable matter ; but as the best results as regards the human voice were obtained from two surfaces of solid gold, I am inclined to view with more favour the idea that an infinite variety of fresh contacts brought into play by the molecular pressure affords the true explanation. It has the advantage of being supported by the numerous forms of microphone I have constructed, in all of which I can fully trace the effect.

I have been very much struck by the great mechanical force exerted by this uprising of the molecules under sonorous vibrations. With vibrations from a musical box 2 feet in length, I found that one ounce of lead was not sufficient on a surface of contact 1 centim. square to maintain constant contact ; and it was only by removing the musical box to a distance of several feet that I was enabled to preserve continuity of current with a moderate pressure. I have spoken to forty microphones at once ; and they all seemed to respond with equal force. Of course there must be a loss of energy in the conversion of molecular vibrations into electrical waves ; but it is so small that I have never been able to measure it with the simple appliances at my disposal. I have examined every portion of my room—wood, stone, metal, in fact all parts—and even a piece of india-rubber : all were in molecular move-

ment whenever I spoke. As yet I have found no such insulator for sound as gutta-percha is for electricity. Caoutchouc seems to be the best; but I have never been able by the use of any amount at my disposal to prevent the microphone reporting all it heard.

The question of insulation has now become one of necessity, as the microphone has opened to us a world of sounds, of the existence of which we were unaware. If we can insulate the instrument so as to direct its powers on any single object, as at present I am able to do on a moving fly, it will be possible to investigate that object undisturbed by the pandemonium of sounds which at present the microphone reveals where we thought complete silence prevailed.

I have recently made the following curious observation :— A microphone on a resonant board is placed in a battery-circuit together with two telephones. When one of these is placed on the resonant board, a continuous sound will emanate from the other. The sound is started by the vibration which is imparted to the board when the telephone is placed on it; this impulse, passing through the microphone, sets both telephone-disks in motion; and the instrument on the board, reacting through the microphone, causes a continuous sound to be produced, which is permanent so long as the independent current of electricity is maintained through the microphone. It follows that the question of providing a *relay* for the human voice in telephony is thus solved.

The transmission of sound through the microphone is perfectly duplex; for if two correspondents use microphones as transmitters and telephones as receivers, each can hear the other, but his own speech is inaudible; and if each sing a different note, no chord is heard. The experiments on the deaf have proved that they can be made to hear the tick of a watch, but not, as yet, human speech distinctly; and my results in this direction point to the conclusion that we only hear ourselves speak through the bones and not through the ears.

However simple the microphone may appear at first glance, it has taken me many months of unremitting labour and study to bring to its present state through the numerous forms each suitable for a special object. The field of usefulness for it widens every day. Sir Henry Thompson has succeeded in applying it to surgical operations of great delicacy; and by its means splinters, bullets, in fact all foreign matter, can be at once detected. Dr. Richardson and myself have been experimenting in lung- and heart-diseases; and although the application by Sir H. Thompson is more successful, I do not doubt that we shall ultimately succeed. There is also hope that

deafness may be relieved. For telephony articulation has become perfect, and the loudness increased. Duplex and multiplex telegraphy will profit by its use; and there is hardly a science, where acoustics has any direct or indirect relation, which will not be benefited. And I feel happy in being able to present this paper on the results obtained by a purely physical action to such an appropriate and appreciative body as the Physical Society.

In conclusion, allow me to state that throughout the whole of my investigations I have used Prof. Bell's wonderfully sensitive telephone instrument as a receiver, and that it is owing to the discovery of so admirable an appliance that I have been enabled to commence and follow up my researches.

VI. *On a Cause for the Appearance of Bright Lines in the Solar Spectrum.* *By* Raphael Meldola, *F.R.A.S., F.C.S., &c.**

IN July 1877 Professor Henry Draper showed that oxygen and (probably) nitrogen are present in the sun's atmosphere, the spectral lines of these gases appearing as bright lines in the solar spectrum.

The photograph accompanying Professor Draper's paper† shows that the oxygen-lines are bright, although not conspicuously so, upon a less-luminous background.

The discoverer of this most important fact in solar chemistry does not offer any complete explanation of the exceptional behaviour of the lines of these elements, but remarks that "it may be suggested that the reason of the non-appearance of a dark line may be that the intensity of the light from a great thickness of ignited oxygen overpowers the effect of the photosphere, just as, if a person were to look at a candle-flame through a yard thickness of ignited sodium-vapour, he would only see bright sodium-lines and no dark absorption-lines. Of course such an explanation would necessitate the hypothesis that ignited gases such as oxygen give forth a relatively large proportion of solar light."

The oxygen-spectrum referred to in the above-mentioned paper is the well-known "line spectrum" seen when powerful disruptive sparks pass through the gas. Dr. Schuster has recently succeeded in obtaining a second or "compound" spectrum of oxygen‡, the fundamental lines of which he has

* Communicated by the Author.

† Nature, vol. xvi. p. 364, August 30, 1877.

‡ Nature, vol. xvii. p. 148, December 20, 1877.

shown with considerable certainty to be present as dark lines in the solar spectrum.

Since the publication of Professor Draper's discovery, I have given much attention to the consideration of a cause for the apparently anomalous brightness of the oxygen-lines; and in the present paper I venture to advance an explanation which has recommended itself as being worthy of notice, not only because it offers a reconciliation of the known solar spectrum with the generally accepted views of the constitution of the sun's atmosphere, but likewise because it furnishes a suggestive hypothesis for the attack of many other obscure problems in solar physics.

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1. I shall throughout this paper consider it to be established that the gaseous envelopes surrounding the sun succeed each other in the following order, commencing with the lowest:—

1. Photosphere.
2. Reversing layer.
3. Chromosphere.
4. Coronal atmosphere.

I also assume the truth of the hypothesis, first advanced by Johnstone Stoney *, who showed, from purely theoretical considerations, that in the sun's atmosphere the various elements must extend to heights which are, broadly speaking, inversely as their vapour-densities. This view has, in my belief, been substantially confirmed by subsequent observation. Thus nitrogen and oxygen, having the respective densities 14 and 16 (H=1), would extend to a great height in the solar atmosphere, rising above sodium, calcium, and magnesium, and having exterior to them the unknown substance giving the D_3 line (helium), hydrogen, and the element giving the coronal line "1474."

2. Two suppositions can be made concerning the sun's temperature. In the first place, it may be assumed that the temperature is so enormously elevated that no chemical compound is anywhere capable of existing in his atmosphere; in other words, dissociation may be considered to be complete. In the next place, it may be supposed that the temperature falls off sufficiently at some region of the outer portion of the sun's atmosphere for certain chemical combinations to take place.

3. Let us first assume that the temperature of the sun is so

* Proc. Roy. Soc. xvi. p. 25, and xvii. p. 1; Phil. Mag. August 1868; Monthly Notices Roy. Astr. Soc. Dec. 1867; Lockyer, Phil. Trans. 1873, vol. clxiii. p. 265.

great that there is perfect dissociation throughout his whole atmosphere. Under these circumstances free oxygen would exist in the presence of electro-positive elements; and, in accordance with Stoney's hypothesis, both this element and nitrogen (if present) would extend to a considerable height in the sun's atmosphere, rising as a necessary consequence, into regions which are cooler than that stratum which is cool enough to reverse the spectral lines of those metals having the smallest molecular mass, viz. Na, Ca, and Mg*. Professor Draper's suggestion that the enormous thickness of incandescent oxygen may overpower the light of the photosphere, can only hold good, when considered in connexion with this hypothesis, if the temperature of the upper portions of the oxygen atmosphere does not differ to any great extent from that of the lower and hotter portions. When, however, we bear in mind the comparatively low vapour-density of oxygen, and consider at the same time to what an enormous height the hydrogen atmosphere extends, it appears probable that the height reached by oxygen would be such that the temperature of the upper portions of this gas would be considerably lower than that of the subjacent layers; so that any excess of radiation over that of the photosphere given out by the hottest portions of the incandescent oxygen would be obliterated by the absorption of the cooler portions above.

[The same reasoning can be applied if we suppose that the temperature of the oxygen falls off at some particular level; so that above this boundary the state of molecular aggregation of the gas corresponds to Dr. Schuster's "compound-line" spectrum, while below this boundary the greater heat of the gas resolves its molecules into the atoms giving the ordinary line-spectrum. The effect of this state of affairs is practically the same as would be brought about by annihilating a certain portion of the upper oxygen layers, since the two different molecular states of the gas give totally dissimilar spectra. We are thus reduced to an oxygen atmosphere of smaller extent, and the foregoing reasoning obtains.]

Angström suggested † that the non-appearance of the lines

* Stoney has shown (Proc. Roy. Soc. xvii. p. 14) that a gas or vapour, even when present in only small quantity, will nevertheless extend to nearly its full height in the solar atmosphere.

† He remarks (*Recherches sur le spectre Solaire*, Upsal, 1869, p. 37) that it is "très-probable que la température élevée du soleil ne suffit pas pour produire les raies brillantes de l'oxygène et de l'azote, et que par conséquent, même en supposant que ces corps existent actuellement dans le soleil, ils ne doivent pourtant pas occasioner de raies obscures dans le spectre solaire." He further suggests that oxygen and nitrogen may exist in the corona.

of oxygen and nitrogen in the solar spectrum might be accounted for by supposing that, at the temperature of the sun, the specific absorptive power of these gases may be insufficient to reverse their spectra. This view, however, equally fails to account for the brightness of the lines in question.

4. Let us now make the not improbable assumption that the temperature of the sun's nucleus, photosphere, and reversing layer is so great that dissociation is perfect throughout these regions, but that somewhere in the higher regions, or above the chromosphere*, the temperature falls off sufficiently for some kinds of chemical combination to take place—say, in the present instance, for oxygen to combine with hydrogen. Under these circumstances we should have, concentric with and exterior to the chromosphere, a zone of combustion where oxygen and hydrogen, already at a very elevated temperature, enter into combination and become thereby raised to a state of more vivid incandescence †. All elements which, by virtue of their small vapour-density, extended into the region of combustion, would be raised to incandescence by contact with the flaming gases, if not actually taking part in the combustion. Thus, according to the present hypothesis, we should not expect to find in the solar spectrum the bright lines of elements having a high vapour-density.

5. The possibility of combination taking place in the higher regions of the sun's atmosphere is admitted by Stoney ‡, who states that "gases in the solar atmosphere which are kept asunder by the temperature of its lower strata may be able to combine in the cooler regions above." Such combination, although arising from the cooling-down of gases previously at a temperature of dissociation, would nevertheless be attended with the evolution of heat, and would possess the character of true combustion. Professor Draper also remarks, in the paper

* It is generally admitted that the true height of the chromosphere is considerably greater than that seen by means of the telespectroscope, since the amount of dispersion necessary to weaken the scattered light of our atmosphere must weaken and shorten the hydrogen-lines by which the chromosphere is revealed.

† It is well known that the oxyhydrogen flame does not show the lines of either of the burning gases. In the sun, however, the conditions are probably very different. The combining gases may be largely diluted with other inactive gases. Furthermore the pressure, as shown by the researches of Frankland and Lockyer (Proc. Roy. Soc. xvii. p. 288), is apparently far less in the upper regions of the chromosphere than in our own atmosphere. Both these causes would conspire to raise the point of ignition of the gases in question, so that a much higher temperature would be necessary to bring about combination than if they were undiluted and under greater pressure.

‡ Proc. Roy. Soc. xvii. p.

before referred to*, that "diffused and reflected light of the outer corona could be caused by such bodies (oxygen compounds) cooled below the self-luminous point."

6. The following considerations appear to give support to the view that oxygen extends into regions sufficiently reduced in temperature for combustion to take place :—

The region which is called the chromosphere is distinguishable as such through what may be called an optical accident: it is that zone of incandescent hydrogen which is rendered visible by the telespectroscope ; the true boundary of the hydrogen atmosphere lies far above the visible chromosphere ; and from this latter zone outwards the temperature falls off rapidly. Now it has been well established by observation, that metals of great molecular mass, such, for example, as those of the iron group, are frequently thrown high up into the chromosphere†. Thus, if gases of great vapour-density are occasionally injected into the chromosphere, gases composed of molecules of comparatively small mass, such as those of oxygen and nitrogen, would probably extend permanently into regions far above the chromosphere, and which are therefore at a much lower temperature than that zone.

The elements chiefly concerned in producing selective absorption in sun-spots, as shown by the local thickening of their spectral lines, are all elements of high vapour-density compared with oxygen—viz. Na, Mg, Ca, Ba, Fe, Ni, Cr, and Ti: from this it appears that the disturbances producing these phenomena must extend low down in the chromosphere. The band-spectra occasionally seen in the nuclei of sun-spots‡ appear to indicate that in these regions the temperature is sometimes sufficiently reduced to admit of the formation of

* *Loc. cit.* p. 366.

† Lockyer, Proc. Roy. Soc. xviii.; Young, Journ. Frank. Inst. Sept. 1869 and Oct. 1870; also 'Nature,' vol. iii. p. 111, and vol. vii. p. 17; Respighi, *Atti d. Real. Accad. d. Linc.* 1872; Tacchini, *Comptes Rendus*, lxxvi. p. 829; H. C. Vogel, *Beobachtungen*, 1872; and numerous other observers.

‡ Professor Young states ('Nature,' vii. p. 109) that in the spectrum of a sun-spot he observed "between C and D some very peculiar shadings, terminated sharply at the less refrangible limit by a hard dark line, but fading out gradually in the other direction at a distance of 3 or 4 Kirchhoff's scale-divisions." This answers in all respects to the spectrum of a compound body: indeed this excellent observer subsequently suggests that these bands "seem to point to such a reduction of temperature over the spot-nucleus as permits the formation of gaseous compounds by elements elsewhere dissociated." In the spectrum of a sun-spot recently observed at the Royal Observatory, Greenwich (Monthly Notices Roy. Astr. Soc. Nov. 9, 1877), a dark-shaded band was seen at about wavelength 6380, "sharp towards the blue and shaded off towards the red. Nothing seen on the sun to correspond with it."

compounds. If therefore the temperature of the solar atmosphere above the spot layer is low enough to permit of chemical combination taking place*, even when the portions of the atmosphere concerned are swept down into the subjacent spot-cavity, it follows that the layer into which oxygen extends (which, as we have seen, must be far above the spot layer) would likewise be cool enough to allow of the formation of compounds.

7. It will help to give greater precision to the hypothesis of a zone of combustion, if we follow the course of a ray of light supposed to be emitted by the photosphere and received in the spectroscope of a terrestrial observer. Passing through the reversing layer, the ray undergoes that selective absorption which gives rise to the Fraunhofer lines ; and if its spectrum could be examined immediately after its emergence from this layer, the oxygen- (and nitrogen-) lines would appear dark, but less conspicuous than the metallic lines, for reasons which will be entered into later on in this paper†. After traversing the chromosphere the ray reaches the zone of combustion, in which region, owing to the increased temperature, the lines of all elements which extend so far would tend to be reversed into bright lines of radiation ‡.

[I say "tend to be reversed," because whether they would actually become so depends upon the specific absorptive power of the elements concerned for the rays in question. Thus, let there be two gases, A and B, of which the spectral lines are A_α, A_β, A_γ and B_α, B_β, B_γ respectively; and let the specific absorptive power of A be greater at a given temperature than

* If these band spectra are regarded as the spectra of elements in the stage of molecular complexity corresponding to the molecule giving the band spectrum of iodine, or Roscoe and Schuster's new spectra of Na and K (Proc. Roy. Soc. xxii. p. 362), the argument remains unaffected, since these band-spectrum-giving molecules are spectroscopically equivalent to the molecules of compound bodies.

† The question here arises as to what order of oxygen-spectrum we should expect to find at the temperature of the reversing layer. Dr. Schuster seems inclined to believe that the temperature may be such as to give the "compound" spectrum of this gas ('Nature,' vol. xvii. p. 148). The recent observations of Lockyer upon the calcium-spectrum (Proc. Roy. Soc. xxiv. p. 352) tend to show that the temperature of this layer is intermediate in dissociation-power between that produced by a small coil with jar and a large coil with jar, a temperature which I am disposed to believe would produce a state of molecular dissociation corresponding to the line-spectrum of oxygen.

‡ It is possible that the temperature of the chromosphere may fall off at some particular level, so as to give above such boundary the "compound" oxygen-spectrum. Should this be the case, the higher portion of the chromosphere may obviously be left out of consideration, so far as relates to its absorbing action on the line-spectrum of oxygen.

that of B. Imagine A and B to be raised to incandescence, and placed in front of a source of white light at a higher temperature, and let this combination be called the "first system." On examination we should see the continuous spectrum crossed by dark lines, A_α, A_β, A_γ, B_α, B_β, and B_γ, of which the first series would be darker than the second. Now conceive the radiation of the whole system to be weakened by general absorption or by removal to a distance. The lines of B would first disappear; so that if we imagine a mixture of A and B ("second system") to be heated to incandescence and placed between the first system and the observer, the B lines might appear bright on a background of continuous spectrum, while the A lines remained dark, although weakened by the radiation of the second layer of mixed gases.]

Thus, if the sun's envelopes exterior to the zone of combustion could be stripped off, we should see the solar spectrum with the lines of oxygen (and nitrogen) bright, and the hydrogen-lines probably dark but much fainter than now seen.

8. The reversal of the oxygen- (and nitrogen-) lines into bright lines by the increased temperature of the region of combustion is rendered possible, even with the intense light of the photosphere as a background [and if, as most probably would be the case, the temperature of the said region of combustion is lower than that of, the photosphere], because the light radiated by the latter has undergone almost its maximum amount of weakening before reaching the zone of combustion, not only on account of the distance of this last region from the photosphere, but also because of the absorption, both selective and general, which the light has undergone in passing through the intervening reversing layer and chromosphere.

9. We have next to turn our attention to that part of the sun's atmosphere exterior to the zone of combustion, in order to account for the fact that the hydrogen-lines appear so intensely dark while the oxygen-lines are bright. The explanation which I venture to suggest is based upon a wide survey of the general spectroscopic characters of the elements.

10. At the temperature of incandescence, the characteristic lines in the spectra of any elements which are compared may be of very different intensities. Thus Cappel has shown *, by a series of quantitative determinations made at the temperature of a Bunsen burner and of an induction-spark, that very different amounts of the metals experimented upon can be detected by means of the spectroscope. The characteristic lines of an element are those which Lockyer has shown to be the *longest*. Interpreting such facts by the aid of the molecular

* Pogg. *Ann.* cxxxix. p. 628 (1870).

theory of gases (and making due allowance for the fact that the characteristic lines of the spectra being compared may occur in parts of the spectrum not *visually* comparable so far as regards intensity), we should say that some kinds of molecules can have certain internal vibrations more readily excited than is the case with other kinds. From the relationship which exists between radiation and absorption, it follows that molecules which have the most sensitive *radiative* organization have likewise the most sensitive *absorptive* organization.

11. The non-metals are distinguished, as a group, from the metals by the greater complexity of their spectra (which more resemble the band spectra of compound bodies), and also by their comparative insensitiveness to the spectroscope. Many of the metals are known to give band spectra at low temperatures; but these break up into line spectra at high temperatures. On the other hand, the band spectra of many non-metals bear temperatures high enough to break up the band spectra of metals without being resolved into line spectra. We might thus have a mixture of two vapours, one metallic and the other non-metallic, at the temperature of incandescence, the former giving a line spectrum and the latter a band spectrum. If we imagine the temperature of such a mixture of vapours to be raised to the point at which the band spectrum of the non-metal breaks up, we should get a line spectrum from both elements; but the metallic lines would be more intense* than those of the non-metal, owing to the greater sensitiveness of the metallic molecule. We should thus have realized the conditions laid down in a former paragraph (7), where A would then represent the metallic, and B the non-metallic vapour.

12. It now remains to show the applicability of the foregoing principles to the case under consideration.

The oxygen and hydrogen of the sun's atmosphere will, for the sake of simplicity, be exclusively considered. These gases represent the metallic and non-metallic vapours of the last paragraph. The photosphere, reversing layer, and chromosphere represent the "first system" of paragraph 7—*i. e.* the

* "In a tube containing both nitrogen and aqueous vapour, the *lines* of hydrogen (spectrum II order) made their appearance at the same time as the spectrum of bands (I order) of nitrogen, whence it follows that the lines of hydrogen are visible in a temperature in which the lines of nitrogen do not appear" (Schellen's 'Spectrum-Analysis,' p. 171). So also Frankland and Lockyer found that in a tube containing hydrogen and nitrogen, the lines of the latter gas under certain conditions of pressure could be made to disappear entirely, while the hydrogen-lines under all conditions remained visible (Proc. Roy. Soc. xvii. p. 454).

source of white light, with the mixture of two vapours of different specific absorptive powers in front. The oxygen and hydrogen of the zone of combustion represent the second layer of incandescent gases of paragraph 7, supposed to have been placed in front of the first system, the total radiation of which is imagined to have been weakened by general absorption or by removal to a distance. It has been shown in paragraph 8 that the total radiation of the photosphere has probably undergone a great amount of weakening from both these causes. Thus the spectrum of a ray which reaches the zone of combustion would exhibit (supposing the zone of combustion and all exterior to it to be stripped off) the lines of oxygen and hydrogen dark, but those of the former much fainter than those of the latter. The action of the incandescent gases of the zone of combustion upon such a spectrum would be to reverse the oxygen lines and to weaken those of hydrogen.

The temperature of the region outside the zone of combustion must fall off, so that any oxygen which might there exist* would be in the state of molecular aggregation corresponding to the compound spectrum, and would thus be without action on the bright-line spectrum of this gas, but would give rise to the dark lines of its compound spectrum. The hydrogen of the region now under consideration by further absorption intensifies the lines of this gas. Thus the solar spectrum as now known is shown to be in complete accordance with the hypothesis here advanced.

The hypothesis of a zone of combustion in the higher regions of the sun's atmosphere, as already stated, furnishes suggegestions for the explanation of many observed facts in solar physics hitherto unaccounted for.

I will first call attention to the intense brilliancy of the line D_3 in the spectrum of the chromosphere, and the extreme faintness of the corresponding dark line in the solar spectrum†. If we consider to what an enormous height this element extends, bearing also in mind that it must consequently reach into comparatively cool regions, and that its radiative (and therefore absorptive) powers are very great, it seems impro-

* It may be supposed that the oxygen atmosphere terminates with the zone of combustion, in which case Dr. Schuster's new oxygen-spectrum must be produced by the absorptive action of the gas in the upper regions of the chromosphere (see also note to paragraph 7).

† This line was seen in July 1877 by H. C. Russell at Sydney. The observer states that "it is a difficult line to see, and only to be made out with high powers." The greatest dispersion of the spectroscope employed was equal to eighteen 61°-prisms (Month. Not. Roy. Astr. Soc. Nov. 9, 1877, pp. 30–32).

bable that the vast thickness of this gas which must be traversed by a ray of light emitted by the photosphere should be barely sufficient to reverse its spectrum. If the existence of a zone of combustion be granted, however, this region becomes the source of radiation of all gases which extend so far. Thus in the case of the D_3 element, which reaches nearly the same level in the sun's atmosphere as hydrogen, the stratum of gas exterior to the zone of combustion is, on the present view, alone concerned in reversing the line under consideration; and this stratum may be of insufficient thickness to produce any marked absorption. The "1474" substance, however, which rises far above hydrogen, appears to exist in sufficient quantity exterior to the supposed region of combustion (or its specific absorptive power is sufficiently great) to produce a marked reversal in the solar spectrum*.

The hypothesis advanced in the present paper does not necessarily imply (at least under existing solar conditions) the production and accumulation of large quantities of compound bodies in the higher regions of the sun's atmosphere. The zone of combustion may be, so to speak, only a local phenomenon confined to a thin shell of the sun's outer envelopes; and compounds formed would be rapidly decomposed both by dissociation and chemical reduction by being swept down into the underlying hotter regions by the convection-currents which take place on such an enormous scale in the sun's atmosphere. The heat of the zone of combustion may also contribute to the dissociation of compounds formed therein †.

* Lockyer has recently shown (*Compt. Rend.* lxxxvi. 319; Proc. Roy. Soc. xxvii. 282) that the blue line of lithium (w.-l. 4603) is represented in the solar spectrum, while the red line (w.-l. 6705) has not hitherto been detected. The question suggests itself whether the absence of this last line may not also be connected with the existence of a region of combustion. The low atomic weight of lithium would lead to the belief that this element extends to a great height in the solar atmosphere. Thus the zone of combustion might be the source of lithium-radiation, and at the temperature of the sun the blue line may be the longest (as appears probable from the fact that this line requires a high temperature for its development); so that the vapour above the region of combustion may be sufficient to reverse the blue, but insufficient to reverse the shorter red line. I would here ask whether the bright red line so frequently seen in the spectrum of the chromosphere by Lockyer (Phil. Trans. 1869, pp. 428 and 429), and described as being less refrangible than C, may not be the missing lithium-line? I may add that a line less refrangible than C has also been frequently seen by Respighi at the base of prominences. It is highly significant that during the eclipse of 1868 a blue line between F and G was seen by Rayet in the spectrum of a prominence. This is the position that would be occupied by the lithium-line w.-l. 4603.

† See Bunsen's experiments on the combustion of different mixtures of CO and H with O (Pogg. *Ann.* cxxxi. 161): also Berthelot "On the Chemical Equilibrium of C, H, and O" (*Bull. Soc. Chim.* [2] xiii. 99).

It is well known to spectroscopists that the solar spectrum is never absolutely free from the so-called "telluric" lines, which have been shown to owe their existence to the aqueous vapour of our atmosphere. It is possible from the present point of view that these lines may be partly caused by aqueous vapour in the higher regions of the sun's atmosphere *. Should there be any connexion between the activity of combustion and the formation of sun-spots, a rigorous comparison of the "telluric" lines in the solar spectrum carefully observed (or still better, photographed) at different periods of the spot-cycle would be of the highest possible interest. Thus it may be suggested that the solar combustion varies periodically in activity—combination being in excess of dissociation during one half of the cycle, and dissociation being in the excess during the other half, when the heat resulting from the combustion, having reached its maximum, tends to decompose the compounds formed. This view points to the belief that the connexion between the sun-spot period and the period of variation of magnetic declination may be due to a common cause—the activity of combustion in the sun's atmosphere and the resulting variation either in the amount of free oxygen, or in the magnetic characters of this gas consequent on variation of temperature.

Sir William Thomson's theory of the dissipation of energy leads to the belief that the sun, like other stars, is gradually cooling down. Thus we should be led to infer *à priori* that there must be a period in the life of a star when compounds can begin to form. Such combination would begin in the outer and cooler portions of the star's atmosphere, as required by the present hypothesis, and would be attended with the development of the heat representing the energy of chemical separation. As the star goes on cooling down, the zone of combustion, at first a mere shell, would gradually encroach upon the central regions, and a star having permanently bright lines in its spectrum would result. In the earlier stages of what may be called the "chemical period" of a star's history—a period into which our sun may be supposed to have entered—the lines of the non-metallic elements would alone appear bright, for the reasons detailed in the foregoing portions of this paper (paragraph 11), and, owing to their comparative faintness, would be lost at the enormous distances which

* I may here recall the much-discussed observation of Secchi, who asserted the existence of water-vapour in the neighbourhood of sun-spots (*Compt. Rend.* lxviii. p. 238). Janssen also, in 1864, observed aqueous vapour in the atmosphere of Antares, and, in 1868, in the atmosphere of many other stars (*Compt. Rend.* lxviii. p. 1845).

the light of the star has to traverse before reaching our spectroscopes. When, however, the region of combustion had encroached sufficiently to reverse the metallic lines, these would shine out with much greater brilliancy than the non-metallic lines, and we should have a background of continuous spectrum crossed by the bright lines of the metals of smallest vapour-density. Such stars would only be expected among those which are, so to speak, in the latest phase of their "chemical period." It is significant that γ Cassiopeiæ, β Lyræ, and η Argo, three stars which show bright lines in their spectra, all have sufficiently complex spectra to warrant the belief that they have entered upon a late phase of their existence. Before the actual reversal of the metallic lines there must exist a period in the life-history of many stars when the temperature and extent of the zone of combustion is such as to obliterate the dark lines of those metals which will ultimately appear as bright lines. Such appears to be the case with the hydrogen in α Orionis; and according to the present views it might perhaps be predicted that this star will sooner or later show a permanent hydrogen-spectrum of bright lines. It is conceivable that in certain cases the composition of a star's atmosphere may be such as to permit a considerable amount of cooling before any combination took place among its constituents; under such circumstances a sudden catastrophe might mark the period of combination, and a star of feeble light would blaze forth suddenly, as occurred in 1866 to τ Coronæ Borealis. In other cases, again, it is possible that the composition of a star's atmosphere may be of such a nature as to lead to a state of periodically unstable chemical equilibrium; that is to say, during a certain period combination may be going on with the accompanying evolution of heat, till at length dissociation again begins to take place. In this manner the phenomena of many variable stars may perhaps be accounted for. On the whole, the possibility of actual combustion taking place in the atmosphere of a slowly cooling star previously at a temperature of dissociation does not seem to me to have had sufficient weight attached to it; and in concluding, I would point out the important factor which is thus introduced into calculations bearing upon the age of the sun's heat in relation to evolution.

London, June 6, 1878.

VII. *Notice of Researches in Thermometry.* *By* Edmund J. Mills, *D.Sc., F.R.S.*[*]

In the course of some researches, commenced some years since, which required a series of accurate measurements with the mercurial thermometer, I had occasion to make a somewhat minute inquiry into the properties of that instrument. The publication of the completely reduced results has been delayed by ill-health and pressure of other work; my present wish is to indicate them, as they may be of interest to those who are engaged in observations of temperature.

I. If an old thermometer be immersed in boiling water, its zero descends. In the course of two or three years, at the ordinary heat of the air, the zero may attain its original position, subject to some slight oscillations according to the season of the year. If x represent the time in months, y the remaining depression, and $(A+B)$ the total depression, the equation to the ascent is

$$y = A\alpha^x + B\beta^x;$$

$A\alpha$ depending on the diameter, $B\beta$ on the length of the bulb. In the case of a spherical thermometer, A is very nearly equal to B. The probable error of a single comparison of theory with experiment, in a fairly favourable instance, does not exceed 0°·01 C.

II. For other temperatures than that of boiling water, other depressions occur; the connexion between these depressions and the temperature seems to follow a compound-interest law, similar to the preceding.

III. If, however, a considerable elevation of temperature be effected, then the zero no longer falls, but rises. In various lead-glass thermometers this phenomenon usually commences at 120°–150°, the bulb collapsing to such an extent as to raise the zero sometimes 8°. At some point, which we may take roughly as not less than 100° higher than the last, the zero is again depressed,—as might, in fact, be expected from the then sensible tension of mercury vapour, aqueous vapour, residual air, and other foreign bodies in the tube.

IV. I have made a large number of comparisons of the mercurial with the air-thermometer. The maximum difference between the two, between 0° and 100°, is at about 33°; neglecting Poggendorff's important correction (as is usually done), it lies at about 50°. It is convenient to use a glass helix, instead of a bulb, for the body of the air-thermometer; in this way convection of air is avoided.

[*] Communicated by the Author.

V. The effect of external pressure on a thermometer's bulb is directly proportional to the pressure as far as about 140 atmospheres. The ascent of the zero of a thermometer on keeping is consequent on a change of state in the glass, being the same whether the thermometer be open or closed, and therefore independent of atmospheric pressure.

VI. When all corrections are made, every individual thermometer has specific characters whereby it differs from all other thermometers.

VII. A number of bodies have been rigorously purified, and their fusion-points determined, with a good second place of decimals, in terms of the air-thermometer: these points range from about 35° to 121°. The possession of these bodies, which can always be preserved without risk, will enable any observer to obtain standard points within that distance, and save a vast amount of tedious experimentation.

Anderson's College, Glasgow.

VIII. *On the Relation between the Notes of Open and Stopped Pipes.* *By* R. H. M. Bosanquet, *Fellow of St. John's College, Oxford.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

It has long been known to practical men that, if an open pipe be stopped at one end, the note of the stopped pipe is not exactly the octave below the note of the open pipe, as it should be according to Bernouilli's theory, but the stopped pipe is somewhat less than an octave below the open pipe; in ordinary organ-pipes the difference is said to be about a major seventh instead of an octave. It has occurred to me lately that the theory of this phenomenon is not generally known; and the following account of it, with some of its applications, may be of interest. I should mention that the investigations were made some time ago, before the publication of my Notes on the Theory of Sound, in the Philosophical Magazine last year; and they were not mentioned there only because the methods depending on them proved of insufficient accuracy for the purpose then in view.

Consider a cylindrical tube open at both ends. Let its length be l, and its diameter 2R. Then the effective (or reduced) length of the pipe is $l+2\alpha$; where α is the correction for one open end, which formed the subject of the investigations contained in Nos. 5 and 6 of my "Notes" (Phil. Mag. [V.] vol. iv. pp. 25, 125, 216).

Now suppose a flat stopper, fitting airtight, to be applied at one end of the tube. It may then, according to the ordinary theory, be regarded as equivalent to the half of an open pipe whose middle point, or node, coincides with the face of the stopper, the effective length measured from the node being $l+\alpha$. The length of the corresponding open pipe would be double of this, or $2(l+\alpha)$. The ratio of the notes is consequently $(l+2\alpha) : 2(l+\alpha)$, which may be put in the form

$$\frac{1}{2} \times \frac{l+2\alpha}{l+\alpha};$$

that is to say, the interval in question differs from an octave by the interval whose ratio is $(l+2\alpha) : (l+\alpha)$.

The following experiment was made with an iron cylindrical tube, 4·9 inches in length and 2 inches in diameter. The notes were determined, as in my former investigations, by blowing short jets of air against the edges. The tube was stopped by standing it upright on a flat surface, and applying a little oil round the edge in contact with the surface. The notes of the pipe, open and stopped, made with one another the interval of a minor seventh; *i. e.* they deviated from the octave by a whole tone. The ratio (9 : 8) was determined with some slight accuracy by comparison with the notes of my enharmonic organ. The tuning of this instrument is not, however, sufficiently stable to base very accurate work on. Then

$$\frac{9}{8} = \frac{l+2\alpha}{l+\alpha},$$

or

$$\frac{1}{8} = \frac{\alpha}{l+\alpha};$$

$$\therefore\ l = 7\alpha.$$

And $l = 4{\cdot}9$ in.,

$$\therefore\ \alpha = {\cdot}7 \text{ in.};$$

and R = 1 in.,

$$\therefore\ \alpha = {\cdot}7\,\mathrm{R}.$$

The value of α for this tube was formerly determined at ·635 R (Phil. Mag. vol. iv. p. 219). The tube has been shortened by about ·1 inch since; but this cannot affect the correction. It appears then that the present process presents general correspondence with the result of the former investigation; but the numerical values of α do not coincide very exactly.

When I originally investigated this subject some time ago, I anticipated that I should be able, by observation of the in-

terval between open and stopped pipes, to determine α in an accurate manner. For this purpose I constructed many pipes, in which the interval in question was as nearly as possible of definite magnitude, generally a semitone less than the octave; but the method proved too inaccurate to be of any real use. An excellent and perfect tonometer is required to measure the intervals accurately; and if we have that, it can be applied to the solution of the problem with greater advantage in other ways. The present method, however, is quite sufficient for the approximate demonstration of the value of α.

There are difficulties in the way of the exact application of these principles to ordinary organ-pipes. First, it is impossible to blow an open and stopped pipe in a similar manner with the same mouthpiece. The pitch varies considerably with the force of the blowing; and the two notes produced with different blowing are not comparable. Again, there is a considerable correction of unknown amount to be taken account of, due to the closing-in of the mouth-end of the pipe.

We may, however, partly get over these difficulties. In the first place, it is possible to arrange a pipe so as to blow the fundamental when open and the twelfth when stopped, without variation of the wind. Secondly, the correction due to the closing-in of the parts round the mouth can be determined for pipes of given shape by sawing one of them across so as to leave a plain circular end. The correction due to the difference in pitch $+\alpha$ (correction for circular end) gives the total value of the correction for the mouth.

The following is an example:—Organ-pipe 9·5 inches from upper lip to open end; diameter ·95 inch. When arranged so as to blow the fundamental when open and twelfth when stopped, the twelfth was 2 commas of the enharmonic organ sharper than the note corresponding to the fundamental. Taking these to be true commas, which they are very nearly, we may take the resulting interval to be 40 : 41.

The correction for the mouth was determined by sawing across a similar pipe; it is roughly

$$\lambda = \frac{l}{6}.$$

Then

$$\frac{41}{40} = \frac{l+\lambda+\alpha}{l+\lambda},$$

$$l+\lambda = 40\alpha,$$

$$\alpha = \cdot 28 \text{ in.};$$

and since

$$2R = \cdot 95 \text{ in.},$$

$$\alpha = \cdot 59\, R \text{ nearly.}$$

This is closer than could be expected considering the extremely rough measurement of the two commas. It will be remembered that the value of α is known to be generally about $\cdot 55\, R$ to $\cdot 6\, R$.

IX. *Notices respecting New Books.*

An Elementary Treatise on Spherical Harmonics and Subjects connected with them. By the Rev. N. M. Ferrers, *M.A., F.R.S., Fellow and Tutor of Gonville and Caius College, Cambridge.* London: Macmillan and Co. 1877. Crown 8vo, pp. 160.

THE author's object in this treatise is "to exhibit, in a concise form, the elementary properties of the expressions known by the name of Laplace's functions, or Spherical Harmonics." More than two fifths of it, comprised in chapters ii. and iii., are devoted to the discussion of the particular case in which the spherical Surface Harmonic (P_i) is a function of μ only. This function Mr. Ferrers calls a Zonal Surface Harmonic; it is the same function as that which Mr. Todhunter calls a "Legendre's Coefficient." The author investigates briefly and elegantly the chief properties of P_i, and then applies them to determine the potential of various forms of attracting matter. Of these the last which he considers is the following comprehensive case:—to find "the potential of a spherical shell of finite thickness whose density is any solid zonal harmonic." These investigations serve as a foundation for those contained in the following chapters. Thus, in the fourth chapter the subject of General, Tesseral, and Sectorial Spherical Harmonics is somewhat briefly treated. It is well known that the general Surface Harmonic of the degree i consists of $2i+1$ terms of the form

$$C \cos \sigma\phi \sin^\sigma \theta \frac{d^\sigma P_i(\mu)}{d\mu^\sigma}, \quad S \sin \sigma\phi \sin^\sigma \theta \frac{d^\sigma P_i(\mu)}{d\mu^\sigma} :$$

to these terms individually Mr. Ferrers gives the name of Tesseral Surface Harmonics of the degree i and order σ; and the last of these terms, viz. those for which $\sigma = i$, he calls Sectorial Surface Harmonics of the degree i. In the fifth chapter he notices very briefly the Spherical Harmonics "of the second kind;" and in the sixth chapter he treats of Ellipsoidal Harmonics, a name which he proposes to give to the functions called by Mr. Todhunter "Lamé's functions."

It is well known that one of the standing difficulties of this subject resides in the proof of the theorem that "any function which does not become infinite between the limits of integration can be expanded in a series of Spherical Harmonics." Thus, Mr. Todhunter notices four or five proofs, and is not, to all appearance, completely

satisfied with any one of them. It may therefore be of some interest to give a sketch of the proof adopted by Mr. Ferrers. The principal discussion takes place with reference to Zonal Harmonics, though a proof of the general case is given on pp. 93–95. The method, then, is as follows:—He *first* obtains an expression for the potential of a spherical shell of uniform small thickness, whose density is A for the part corresponding to the value of μ from 1 to λ, and B for the part corresponding to the value of μ from λ to -1. He then extends the same method to the case in which the densities are A from $\mu=1$ to $\mu=\lambda_1$, B from $\mu=\lambda_1$ to $\mu=\lambda_2$, and C from $\mu=\lambda_2$ to $\mu=-1$. His next step is to take $\lambda_1=\lambda$ and $\lambda_2=\lambda+d\lambda$, and thus arrives at the result that

$$\tfrac{1}{2}d\lambda\{1+\ldots+(2i+1)\mathrm{P}_i(\lambda)\mathrm{P}_i(\mu)+\ldots\}$$

has the value unity from $\mu=\lambda$ to $\mu=\lambda+d\lambda$, and zero for other values of μ. Now if we suppose λ_1, λ_2, ... to be continuous values of λ ranging from 1 to -1, it is evident that the expression

$$\tfrac{1}{2}d\lambda\,.f(\lambda_1)\{1+\ldots+(2i+1)\mathrm{P}_i(\lambda_1)\mathrm{P}_i(\mu)+\ldots\}+$$
$$\tfrac{1}{2}d\lambda\,f(\lambda_2)\{1+\ldots+(2i+1)\mathrm{P}_i(\lambda_2)\mathrm{P}_i(\mu)+\ldots\}+\ldots$$

will equal $f(\lambda_1)$ when $\mu=\lambda_1$, $f(\lambda_2)$ when $\mu=\lambda_2\ldots$; and consequently the whole expression must be $f(\mu)$. But it is also plain that

$$\{f(\lambda_1)\mathrm{P}_i(\lambda_1)+f(\lambda_2)\mathrm{P}_i(\lambda_2)+\ldots+\}d\lambda=\int_{-1}^{1}f(\mu)\mathrm{P}_i(\mu)d\mu,$$

and consequently that $f(\mu)$ can be expressed as a series whose general term is

$$\tfrac{1}{2}(2i+1)\mathrm{P}_i(\mu)\int_{-1}^{1}\mathrm{P}_i(\mu)f(\mu)\,.\,d\mu,$$

the theorem to be proved.

Instructions to Meteorological Observers in India, being the First Part of the Indian Meteorologist's Vade Mecum. By Henry F. Blanford, *Meteorological Reporter to the Government of India.* Calcutta, 1876.

Meteorology of India, being the Second Part of the Indian Meteorologist's Vade Mecum. By Henry F. Blanford. Calcutta, 1877.

Tables for the Reduction of Meteorological Observations in India. By Henry F. Blanford. Calcutta, 1876.

The works above mentioned constitute a complete Meteorological Library, and, although written expressly for India, are by no means restricted in interest to the Indian Peninsula, as they contain remarks of general application, especially relative to the instruments employed in Meteorological research. In the "Instructions" each instrument, including its varieties, is fully described; and the reader will find many valuable hints on the use of the Barometer, Thermometer, Actinometer, Hygrometer, Rain-gauge, Wind-vane, and Anemometer; also interesting articles on Cloud and Weather obser-

vations, with Hours and Reduction of Observations, the latter treating very fully of the most recent and exact methods employed. In the "Notes on Registration" and "Rules for Observers" the amateur will find much important information.

The second part of the Vade Mecum is a very valuable production: the portions of greater interest treat of the physical properties of air and vapour and the physical geography of India; the peculiar conformation of the surface of India, which is lucidly described, renders the country in relation to Meteorology an epitome of atmospheric physics. Students will derive much useful information from their conjoint study. The succeeding portions, on Temperature, Pressure, Wind and Rain, are full of important information; and the concluding part, on Storms, contains the most recent development of the Theory of Cyclones, especially the incurvature of the wind's motion in storms.

X. *Proceedings of Learned Societies.*

GEOLOGICAL SOCIETY.

[Continued from p. 473.]

March 20, 1878.—Henry Clifton Sorby, Esq., F.R.S., President, in the Chair.

THE following communications were read:—

1. "On the Chronological Value of the Triassic Strata of the South-western Counties." By W. A. E. Ussher, Esq., F.G.S.

The author maintained that the general thinning-out of the Trias in the South-Devon and West-Somerset area as it is traced northward, of which he adduced evidence, proves that this area was not connected with that of Gloucestershire and the midland counties until the later stages of the Keuper; and endeavoured to show by a comparison of sections that the area east of Taunton and south of the Mendips was not submerged before the deposition of the Lower Keuper Sandstone, and probably not until the later stages of its formation, the Quantocks acting as a barrier dividing the Bridgewater area from the Watchet valley. He thought that a subsidence progressing from south to north led to earlier deposition in South Devon, and to a consequent attenuation of the lower beds towards Watchet and Porlock. Hence the lowermost beds of the Trias of the south coast are much thicker than their more northerly equivalents, and probably were still thicker where the English Channel now flows, some beds perhaps dating as far back as Permian times. The presence of numerous fragments of igneous rocks (quartz-porphyries) in the basement-beds of the South-Devon Trias, and the absence of known corresponding rocks in the county, led the author to infer that the cliffs and beds of the early Triassic sea were composed of such rocks, any undestroyed portions of which would probably occur either under the Triassic beds near Dartmoor and between Newton and Seaton, or in the area now occupied by the

English Channel. As continuity is evident only in the upper division of the Trias, between the area of Devon and Somerset and that of the midland counties, and there is no unconformity in the former, the author maintained that the upper marls, upper sandstones, and probably the conglomerate and pebble-bed subdivision of Devon and Somerset, are equivalent in time to the Keuper series of the Midland counties, and that deposition took place in Devon and Somerset between Keuper and Bunter times, bridging over the hiatus marked by unconformity in the Midland counties.

2. "Note on an *Os articulare*, presumably that of *Iguanodon Mantelli*." By J. W. Hulke, Esq., F.R.S., F.G.S.

3. "Description of a new Fish from the Lower Chalk of Dover." By E. Tulley Newton, Esq., F.G.S.

4. "Further remarks on adherent Carboniferous Productidæ." By R. Etheridge, jun., Esq., F.G.S.

5. "The Submarine Forest at the Alt Mouth." By T. Mellard Reade, Esq., F.G.S.

April 3.—Henry Clifton Sorby, Esq., F.R.S., President, in the Chair.

The following communications were read:—

1. "On an Unconformable Break at the base of the Cambrian Rocks near Llanberis." By George Maw, Esq., F.L.S., F.G.S.

In a paper read before the Society on December 5, 1877 (Q. J. G. S. vol. xxxiv. p. 137), Prof. Hughes referred to an observation made by the author in 1867 as to the occurrence near Llanberis of an unconformable break, indicating the base of the Cambrian, and, while accepting the asserted existence of pre-Cambrian rocks in North Wales, placed the base of the Cambrian in a very different position, and maintained that the appearances described by Mr. Maw might be accounted for by lateral pressure acting upon beds of dissimilar texture and unequal hardness. The author had reexamined the section in question, and maintained his original interpretation of the phenomena, which he regarded as the earliest indication of the existence of a pre-Cambrian series. He accounted for differences observed in the supposed pre-Cambrian rocks at Moel Tryfaen and Llanberis by regarding them as having undergone different degrees of metamorphism.

2. "On the so-called Greenstones of Central and Eastern Cornwall." By J. Arthur Phillips, Esq., F.G.S.

In this paper the author extended his investigations of the rocks formerly mapped as greenstones, from the western (see Q. J. G. S. vol. xxxii. p. 155) into the central and eastern districts of Cornwall. He described in detail various rocks from different parts of these districts, the examination of which had led him to the following conclusions. The numerous lavas which occur here, in addition to the rocks met with in Western Cornwall, are so interbedded with the slates and schists as to lead to the conviction that they are con-

temporaneous; and, although much altered, they closely resemble lavas of more modern date. Sometimes they assume a distinctly schistose character. The crystalline greenstones are more varied and instructive than those of the western portion of the county; some of them are typical dolerites, while others are so altered as to consist only of a granular indefinite base, traversed by indistinct microlitic bodies. Their pyroxenic constituent is augite; and although many would call them diabases or melaphyres, the author regards it as more logical to regard them as ancient dolerites. Where these rocks are altered the augite is usually changed into hornblende and viridite, while the felspar becomes cloudy, and finally merges into a granular base. The crystals of augite are often gradually replaced by an assemblage of felted microlites; in other cases their outlines are preserved whilst their substance is replaced by hornblende, the rock being thus converted into a *uralite-dolerite* or *uralite-diabase*. When these rocks do not contain augite, and are to a great extent composed of long bacillary hornblendic crystals made up of parallel belonites, the ends of which are frequently curved outwards, it is probable that hornblende was an original constituent of the rock, which is therefore a true diorite. Slaty or schistose greenstones are less frequent than in the western districts: but on St.-Cleer Down the "hornblende-slates" graduate imperceptibly from crystalline dolerites into clay-slate: these are not improbably of igneous origin. Some of the slaty blue elvans are identical in chemical composition with the dolerites, and may be highly metamorphosed ash-beds, although, from some of their characters, it seems more probable that they are true igneous rocks. The felspar in the brecciated slates is almost entirely plagioclase, and is derived from the disintegration of greenstones. With regard to the age of the rocks described, the author states that they are generally older than the granite; for the vesicular lavas and many slaty hornblendic bands are evidently contemporaneous with the slates among which they are bedded, while the latter are often displaced by the granite or traversed by granitic veins; and, further, the eruptive doleritic rocks which break through the sedimentary beds never traverse the granite, but are often interrupted by it.

3. "The Recession of the Falls of St. Anthony." By N. H. Winchell, Esq. Communicated by J. Geikie, Esq., F.R.S., F.G.S.

The author's purpose in this paper was to arrive at an estimate of the date of the last glacial period from the rate of recession of the Falls of St. Anthony, near the junction of the Minnesota and Mississippi rivers. He stated that the country is covered with deposits of glacial origin, that between the present falls and Fort Snelling, a distance of eight miles, the existing river-gorge has been formed since the deposition of the newer Boulder-clay, and that the old river-valley is filled up with glacial deposits. The gorge is of very uniform character, being cut through hard limestones resting on soft sand rock, both lying quite horizontally. The country was settled in 1856; and the recession of the falls has since been very

rapid, its rate having been accelerated by the erection of saw-mills, dams, &c. From the accounts of various travellers who have visited the falls in the last 200 years, the author endeavoured to obtain an estimate of the true rate of recession. Between the visit of Father Hennepin in 1688 and that of Carver in 1766 he finds a recession at the rate of 3·49 feet annually, between Carver's visit and 1856 a mean annual recession of 6·73 feet, and between Hennepin and 1856 one of 5·15 feet. The time-estimates for the cutting of the gorge would be, according to the above means, 12, 103, 6, 276 and 8202 years. The author considers the data upon which the second of these numbers is founded the most reliable.

April 17.—Henry Clifton Sorby, Esq., F.R.S., President, in the Chair.

The following communications were read:—

1. "On the Geological results of the Polar Expedition under Admiral Sir George Nares, F.R.S." By Capt. H. W. Feilden, R.A., F.G.S., and C. E. De Rance, Esq., F.G.S.

The authors describe the Laurentian gneiss that occupies so large a tract in Canada as extending into the Polar area, and alike underlying the older Palæozoic rocks of the Parry Archipelago, the Cretaceous and Tertiary plant-bearing beds of Disco Island, and the Oolites and Lias of East Greenland and Spitzbergen. Newer than the Laurentian, but older than the fossiliferous rocks of Upper Silurian age, are the Cape-Rawson beds, forming the coast-line between Scoresby Bay and Cape Cresswell, in lat. 82° 40′; these strata are unfossiliferous slates and grit, dipping at very high angles.

From the fact that Sir John Richardson found these ancient rocks in the Hudson's-Bay territory to be directly overlain by limestones, containing corals of the Upper Silurian Niagara and Onondaga group, Sir Roderick Murchison inferred that the Polar area was dry land during the whole of the interval of time occupied by the deposition of strata elsewhere between the Laurentian and the Upper Silurian: and the examination by Mr. Salter, Dr. Haughton, and others of the specimens brought from the Parry Islands have hitherto been considered to support this view. The specimens of rocks and fossils, more than 2000 in number, brought by the late expedition from Grinnell and Hall Lands have made known to us, with absolute certainty, the occurrence of Lower-Silurian species in rocks underlying the Upper Silurian; and as several of these Lower-Silurian forms have been noted from the Arctic Archipelago, there can be little doubt that the Lower Silurians are there present also. The extensive areas of dolomite of a creamy colour discovered by M'Clintock around the magnetic pole, on the western side of Boothia, in King William's Island, and in Prince-of-Wales Land, abounding in fossils, described by Dr. Haughton, probably represent the whole of the Silurian era and possibly a portion of the Devonian.

The bases of the Silurians are seen in North Somerset, and

consist of finely stratified red sandstone and slate, resting directly on the Laurentian gneiss, resembling that found at Cape Bunny and in the cliffs between Whale and Wolstenholme Sounds. Above these sandstones occur ferruginous limestones, with quartz grains: and still higher in the series the cream-coloured limestones come in. The Silurians occupy Prince-Albert Land, the central and western portion of North Devon, and the whole of Cornwallis Island. The Carboniferous Limestone was discovered, rising to a height of 2000 feet, on the extreme north coast of Grinnell Land, in Feilden and Parry Peninsulas, and contains many species of fossils in common with the rocks of the same age in Spitzbergen and the Parry Archipelago, being probably continuously connected with the limestone of that area, by way of the United-States range of mountains. The coal-bearing beds that underlie the Carboniferous Limestones of Melville Island are absent in Grinnell Land; but they are represented by true marine Devonians, established in the Polar area for the first time, through the determination of the fossils, by Mr. Etheridge. In America a vast area is covered by Cretaceous rocks. The lowest division, the Dakota group, contains lignite seams and numerous plant-remains indicating a temperate flora; overlying the Cretaceous series are various Tertiary beds, each characterized by a special flora, the oldest containing subtropical and tropical forms, such as various palms of Eocene type. In the overlying Miocene beds the character of the plants indicates a more temperate climate; and many of the species occur in the Miocene beds of Disco Island, in West Greenland, and a few of them in beds associated with the 30-feet coal-seam discovered at Lady-Franklin Sound by the late expedition. The warmer Eocene flora is entirely absent in the Arctic area; but the Dakota beds are represented by the "Atane strata" of West Greenland, in which the leaves of dicotyledonous plants first appear. Beneath it, in Greenland, is an older series of Cretaceous plant-bearing beds, indicating a somewhat warmer climate, resembling that experienced in Egypt and the Canary Islands at the present time. In the later Miocene beds of Greenland, Spitzbergen, and the newly discovered beds of Lady-Franklin Sound, the plants belong to climatal conditions 30° warmer than at present, the most northern localities marking the coldest conditions. The common fir (*Pinus abies*) was discovered in the Grinnell-Land Miocene, as well as the birch, poplar, and other trees, which doubtless extended across the polar area to Spitzbergen, where they also occur.

At the present time the coasts of Grinnell Land and Greenland are steadily rising from the sea, beds of glacio-marine origin, with shells of the same species as are now living in Kennedy Channel, extending up the hillsides and valley-slopes to a height of 1000 ft., and reaching a thickness of from 200 to 300 ft. These deposits, which have much in common with the "boulder-clays" of English geologists, are formed by the deposition of mud and sand carried down by summer torrents and discharged into fiords and arms of the sea, covered with stone- and gravel-laden floes, which, melted by the heated and turbid waters, precipitate their freight on the mud

below. As the land steadily rises these mud-beds are elevated above the sea. The coast is fringed with the ice-foot, forming a flat terrace 50 to 100 yards in breadth, stretching from the base of the cliffs to the sea-margin. This wall of ice is not made up of frozen sea-water, but of the accumulated autumn snowfall, which, drifting to the beach, is converted into ice where it meets the sea-water which splashes over it.

2. "On the Palæontological results of the recent Polar Expedition under Admiral Sir George Nares, K.C.B., F.R.S." By Capt. H. W. Feilden, R.A., F.G.S., and Robert Etheridge, Esq., F.R.S., F.G.S.

XI. *Intelligence and Miscellaneous Articles.*

AMMONIO-ARGENTIC IODIDE. BY M. CAREY LEA.

WHEN silver iodide is exposed to ammonia-gas it absorbs 3·6 per cent., and forms, according to Rammelsberg, a compound in which an atom of ammonia is united to two of AgI. Liquid ammonia instantly whitens AgI: every trace of the strong lemon-yellow colour disappears. The behaviour of the ammonia iodide under the influence of light differs singularly from that of the plain iodide, and will be here described.

The affinity of AgI for ammonia is very slight. If the white compound be thrown upon a filter and washed with water, the ammonia washes quickly out, the yellow colour reappearing. If simply exposed to the air, the yellow colour returns while the powder is yet moist; so that the ammonia is held back with less energy than the water. So long, however, as the ammonia is present, the properties of the iodide are entirely altered.

AgI precipitated with excess of KI does not darken by exposure to light even continued for months. But the same iodide exposed under liquid ammonia rapidly darkens to an intense violet-black, precisely similar to that of AgCl exposed to light, and not at all resembling the greenish-black of AgI exposed in presence of excess of silver nitrate. (This difference no doubt depends upon the yellow of the unchanged AgI mixing with the bluish-black of the changed, whereas in the case of the ammonia iodide the yellow colour has been first destroyed.)

When the exposure is continued for some time, the intense violet-black colour gradually lightens again, and finally quite disappears; the iodide recovers its original yellow colour, with perhaps a little more of a greyish shade. This is a new reaction, and differs entirely from any thing that has been hitherto observed. It has been long known that darkened AgI washed over with solution of KI and exposed to light, bleached. This last reaction is intelligible enough; for KI in solution exposed to light decomposes, and in presence of AgI darkened by light gives up iodine to the AgI, and so bleaches it. The above experiment is quite different. The darkened substance may be washed well with water (during which

operation it passes from violet-black to dark brown), and may then be exposed to light either under liquid ammonia or under pure water: in either case the bleaching takes place, though in the latter case more slowly.

If the experiment be performed in a test-tube, the bleaching under ammonia requires several hours, under water from one to three days. But if the iodide be formed upon paper, and this paper be exposed to light, washing it constantly with liquid ammonia, the darkening followed by the bleaching requires little more than a minute. In this case, however, the bleaching is not so complete, perhaps because of the influence of the organic matter present. The bleaching appears to depend upon the escape of ammonia; for if the darkened ammonia iodide is covered with strong liquid ammonia and the test-tube well corked, the bleaching does not take place.

It became a matter of interest to know whether the darkening under ammonia was accompanied by any decomposition—whether the ammonia took up iodine from the silver salt under the action of light. For this purpose AgI was precipitated with excess of KI, and subjected to a long and thorough washing; it was then exposed for several days to light under strong liquid ammonia. As AgI is not wholly insoluble in ammonia, the mother-water was first evaporated to dryness at a heat but little over ordinary temperatures. The traces of residue were washed with water; and this water gave distinct indications of iodine. The iodine present is in so small a quantity that it may easily be overlooked: but it is certainly there. The washing given to the AgI was so thorough that it seemed impossible to admit that traces of KI remained attached to the AgI; but in order to leave no room for doubt, the experiment was repeated, using an excess of silver nitrate in making the precipitation, followed by thorough washing. Iodine was still found in combination with ammonia; and under these conditions there could be no doubt that AgI had been decomposed.

When AgI is blackened under ammonia in a test-tube, and the uncorked test-tube is set aside in the dark for a day or two, the AgI assumes a singular pinkish shade. It thus appears that AgI under the influence of ammonia and of light gives indications of most of the colours of the spectrum. Starting with white, it passes under the influence of light to violet, and thence nearly to black: this violet-black substance washed with water passes to brown. The brown substance covered with ammonia and left to itself in an open test-tube becomes pinkish in the dark, yellow in sunlight. These curious relations to colour which we see in the silver haloids, from time to time exhibiting themselves in new ways, seem to give hope of the eventual discovery of some complete method of heliochromy.—Silliman's *American Journal*, May 1878.

Philadelphia, March 25, 1878.

ON THE PRODUCTION OF PLATEAU'S FILM-SYSTEMS.

BY A. TERQUEM.

M. Plateau, by the use of a mixture of soap-water and glycerine, has produced liquid films of a certain extent, and has thus been able to verify most of the laws respecting the form of the surfaces which constitute the boundaries of liquids whose molecules are subjected only to their reciprocal actions.

I pointed out, some years since, that for the glyceric liquid a mixture of soap-water and sugar might be substituted, the latter substance having, especially, like glycerine, the effect of augmenting the viscosity of the liquid, and preventing it from flowing away too rapidly. The production of the film-systems of M. Plateau demands the employment of a great quantity of liquid if polyhedra be used the edges of which are of large dimensions.

I have thought that large films of liquid might be easily obtained by bounding them in part by flexible threads instead of using for the purpose rigid wires only.

Thus, if two horizontal rods be joined by two vertical and equidistant flexible threads, on dipping the system in the saponaceous liquid and slowly lifting it out again, we get a vertical plane film bounded above and below by the two rods, and laterally by the flexible threads, which take the form of arcs of a circle. The radius of the circle evidently depends on the stretching weight. It is easily demonstrated that, if we designate by l the distance between the two threads, by R the radius of the arc constituted by them, by ϕ the angle made with the vertical by the tangent of the arc at the point of attachment of the thread to the lower rod, by f the superficial tension of each of the two surfaces of the liquid film, and by p the stretching weight, we have the relation

$$p=2f(l+2\mathrm{R}\cos\phi).$$

Every thing happens, therefore, as if the distance between the threads were equal to that between the centres of the two arcs, the tension of the threads being omitted.

I of course submitted this formula to a series of experimental verifications, by measuring with the cathetometer the diminution of the vertical distance between the two horizontal rods produced by the existence of a liquid-film between them.

If H is the initial length of the threads, and H′ the new vertical distance of the horizontal rods, to find the radius of the circle and the angle ϕ we have only to solve the two equations

$$\mathrm{H}'=2\mathrm{R}\sin\phi \text{ and } 2\mathrm{R}\phi=\mathrm{H},$$

whence we derive

$$\mathrm{H}'\phi=\mathrm{H}\sin\phi.$$

As the angle ϕ is generally very small, this transcendental equation can be solved with sufficient approximation by substituting $\phi-\frac{\phi^3}{6}$ for the sine.

Proceeding thus, I have obtained, as the mean of a number of determinations, the number 2·79 milligrams for the value of f. On the contrary, measuring the superficial tension by employing the drop-counter, I obtained the value 3·47 milligr., considerably higher than the preceding. This difference, which surprised me at first, too great to be attributed to errors of experiment, would need to be controlled by other similar determinations made upon other liquids, of which there are but few capable of forming films of large extent. It may have been due, according to M. Duclaux (to whom I submitted the difficulty), to the circumstance that in a very thin film like that of soap-solution, the superficial tension has a lower value than in the free surface of the same liquid in indefinite mass —which would indicate that the thickness of the liquid layer in which the molecules have the abnormal arrangement which produces superficial tension exceeds half the thickness of the soap-film.

Thanks to the same arrangement, I have been able to execute other experiments suitable for showing the superficial tension of liquids. Moreover, on replacing, in Plateau's polyhedra (such as the tetrahedron, the cube, the octahedron, &c.), a certain number of rigid rods by flexible threads, we can produce with great facility, and with a minimum quantity of liquid, laminar systems of considerable dimensions and a certain number of surfaces possessing the characteristic property that their mean curvature is *nil*.—*Comptes Rendus de l'Académie des Sciences*, April 29, 1878, tome lxxxvi. pp. 1057, 1058.

ON THE MAGNETIC ROTATION OF THE PLANE OF POLARIZATION OF LIGHT UNDER THE INFLUENCE OF THE EARTH. BY HENRI BECQUEREL.

In the course of my investigations on magnetic rotatory polarization, I have been led to the direct estimation of the action of terrestrial magnetism upon various substances. This action can be very neatly made evident by an experiment which seemed to me sufficiently interesting to be communicated to the Academy.

Between a Jellet polarizer and an analyzer furnished with a telescope and mounted on a divided circle is placed a tube of half a metre length, terminated by parallel glass plates and containing bisulphide of carbon. At the two ends of the tube, plane mirrors arranged as Faraday arranged them gave several successive reflections of the luminous ray, and thus augmented the observed rotation. In the present experiment the second reflection could be viewed; the ray had therefore traversed the tube five times, corresponding to a thickness of 2·5 metres of bisulphide of carbon. The source of light was a pipe with oxyhydrogen gas. A great amount of light was lost by absorption and by the successive reflections; and the rays transmitted to the eye were chiefly the yellow rays. The entire system was firmly fixed to a horizontal copper rule, and

could turn about a vertical axis so as to orient the luminous beam in various directions.

With this arrangement it is ascertained that, if the system be made to coincide in direction with the plane of the magnetic meridian, the same position of the polarization-plane is not obtained whether we look towards the south or towards the north; a great number of closely concordant measurements gave an angular difference of about 6′·5 between the two positions. On the contrary, if we place ourselves in a position perpendicular to the magnetic meridian, we get the same direction of the plane of polarization whether we look towards the east or towards the west; and this position is the bisectrix of that which we have in viewing towards magnetic north and south.

It may hence be concluded that the angular difference observed is a rotation of the plane of polarization of the light, due to the action of the earth; the number 6′·5 measures the double of the rotation for yellow light and in the special conditions of the experiment. The direction of this rotation is the same as that of the rotation of the earth; it is the direction of an electric current which, on the hypothesis of Ampère, would give rise to the phenomena of terrestrial magnetism.

It must be remarked that the number we have given only refers to observations made in the laboratory of the Muséum d'Histoire naturelle, in proximity to more or less considerables masses of iron. To ascertain with more precision the action of the globe, and to utilize this method for estimating the intensity of the earth's magnetism, it would be necessary to use the same precautions as for ordinary observations of terrestrial magnetism, and to amplify the phenomenon by taking a longer tube. This is what I am at present engaged in.

The system arranged as above indicated exhibits remarkable sensitiveness to the action of magnetism: an ordinary bar magnet, held in the hand, and brought parallel near to the tube, first in one direction, then in the other, is sufficient to make manifest a rotation of the plane of polarization that may attain to upwards of 1°.

It is interesting to compare this direct measurement with an estimation, made by Mr. Gordon*, of the magnetic rotation produced by 1 centim. of bisulphide of carbon under a magnetic action equal to unity. The result found by calculating from this number the action of the terrestrial horizontal component is, that 2·5 metres of bisulphide of carbon should give, with yellow light, a single rotation of 3′·8—instead of 3′·25, which results from our direct observation. The difference may be due to exterior perturbations. If we adopt the latter number, we see that, under the conditions in which we have placed ourselves, the double rotation of 1 metre of bisulphide of carbon would be 2′·6, and that of 1 metre of water 0′·8.—*Comptes Rendus de l'Académie des Sciences*, April 29, 1878, tome lxxxvi. pp. 1075–1077.

* Philosophical Transactions, 1877, Part I.

ON THE CRYSTALLIZATION OF SILICA IN THE DRY WAY.

BY P. HAUTEFEUILLE.

In 1868 M. vom Rath described, in a trachyte, some small lamelliform crystals, of which he made a new species, tridymite. The observations of that accomplished mineralogist prove that tridymite is silica crystallized under a form different from that of quartz, and possessing a lower density than that species. Like sulphur, arsenious acid, flowers of antimony, &c., silica crystallizes under two incompatible forms. It can be prepared, as I shall prove, in the dry way, under both these forms.

The only known process for crystallizing silica in the dry way we owe to M. G. Rose; it is based on the employment of the phosphorus-salt, and permits only tridymite to be prepared. The alkaline tungstates can, with advantage, be substituted for the phosphates; for they permit crystallized silica to be obtained, at pleasure, either under the form of tridymite or under that of quartz.

Reproduction of Tridymite.—Amorphous silica, kept at the melting temperature of silver in tungstate of sodium, crystallizes in a few hours. After the cooling, treatment with water dissolves the alkaline tungstate and lays bare a crystalline sand, the weight of which is, within a few thousandths, that of the silica employed.

The principal crystallographic and optical characters of tridymite are easily verified in the crystals obtained by this process. They are thin hexagonal scales, mostly piled one upon another to the number of three or four*. Upon the most regular scales one or two half-scales are frequently lodged.

By the long-continued action of tungstate of sodium at a nearly constant temperature of 1000° C., the tridymite can be obtained in thick scales. These crystals, mixed with large lamellæ grouped according to one of the two laws alluded to, are hexagonal tables with faces free from striæ†.

A pencil of parallel-polarized light is not depolarized when it passes quite perpendicularly through these hexagonal scales, whatever the thickness.

The density of the crystals prepared in platinum vessels, with pure tungstate of sodium, and with silica containing neither alumina, nor oxide of iron, nor magnesia, is 2·30 at 16° C.; Vom Rath, operating on some crystals containing about 2 per cent. of oxides, found 2·326, 2·312, and 2·296 for the density at 16°. The determination made upon an absolutely pure product proves that silica in the form of tridymite possesses a density certainly intermediate between those of quartz (2·65) and fused silica (2·20).

Tridymite is more readily attacked than quartz by reagents both in the wet and the dry way. Tungstate of sodium itself can destroy the tridymite. Thus, at a temperature much above 1000°,

* These scales are often partly corroded, like those met with in the trachytes of Mont Dore.

† The ratio $\frac{h}{b}$ appears to be constant and equal to $\frac{1}{2}$.

there is formed at the expense of the lamelliform crystals a silicate disseminated in droplets in the fused salt. This destruction of the tridymite is only temporary, if the silicate be maintained in the liquid bath formed by the acid tungstate, at a temperature between 900° and 1000°; for this silicate then undergoes a decomposition which regenerates the tridymite. This sort of precipitation in the dry way demands much more time than the crystallization of amorphous silica. This is why it is more advantageous to heat silica with tungstate of sodium than to decompose an alkaline silicate by tungstic acid.

The destruction of tridymite, and subsequent precipitation of silica in the form of lamellæ, permit the part played by the tungstate of sodium in the act of crystallization to be analyzed. The alkali of the tungstate attacks the silica, producing an alkaline silicate; and the tungstic acid retakes at a lower temperature the alkali which the silica had taken from it. These two reciprocally inverse actions take place successively when the temperature oscillates between certain limits. They suffice to explain the crystallization, without it being necessary to invoke the solubility of silica in the fused salt; for these reactions are perfectly comparable with those which determine the crystallization of the sesquioxide of iron when it is heated in gaseous hydrochloric acid.

Although I cannot at present compare the results furnished by the method I have just made known with those which can be obtained by a judicious application of M. G. Rose's method, I will notice this fact, that the preparation of tridymite by means of tungstate of sodium does not require a temperature so elevated as that which is necessary when acid phosphate of sodium is employed. The phosphoric salt and the tungstate of sodium are both mineralizers of silica; but the latter salt exerts a more energetic action than the former, even at a less-elevated temperature—which permits it to be employed for reproducing the numerous more or less fusible silicates which are associated with silica in the rocks.—*Comptes Rendus de l'Académie des Sciences*, May 6, 1878, tome lxxxvi. pp. 1133–1135.

OUTLINES OF THE ACTINIC THEORY OF HEAT.

BY PROF. C. PUSCHL.

In the present form of the mechanical theory of heat, and specially in the kinetic theory of gases, the sum of the *vires vivæ* of the ponderable atoms of a body in motion among themselves is regarded as the quantity of heat of the body; while the sum of the *vires vivæ* of the æther contained in the body is neglected, and it is assumed that this medium exerts no sensible influence on the motions of the atoms. This presentation the author holds to be inadequate. It is remarked that a hot body may be cooled not merely by contact with colder bodies, but also by radiation, and that the velocity of such a cooling is not always inconsiderable, but under favourable circumstances is so great that many physicists

occasionally note it as astonishing. It is evident that the atoms of a heated body give up *vis viva* to the æther ; in other words, the æther takes from the moving atoms of the body a part of their velocity ; it therefore, indeed, exerts upon them an influence which acts as a resistance checking their velocity, and is by no means inconsiderable, but of relatively great intensity. Conversely, a body can receive heat not merely by contact with warmer bodies, but also by radiation from without. Its atoms then take *vis viva* from the æther ; that is, the æther imparts to the atoms an increase of velocity. Consequently, in a body cooled by radiation outwards, the motions of its atoms are rendered slower by the æther ; in one growing warmer through radiation from without the motions of its atoms are accelerated ; in a body which under equal radiation outwards and inwards maintains a constant temperature the forces of the æther retarding and accelerating the motions of its atoms must on the whole maintain equilibrium ; and in consequence of the rapid change of atom-motion into radiant heat, and of radiant-heat into atom-motion, these forces must, for every temperature that occurs, possess great intensity. The *kinetic* theory, which takes no account of such forces, is, according to the author, incapable of anyhow rendering intelligible a rapid annihilation or generation of atomic velocities by mere generation or annihilation of æther-undulations, whereas to him the *actinic* theory, touched upon in the present memoir, appears to correspond perfectly with the above conclusion. According to it an essential portion of the heat of a body consists of radiating heat, which is accumulated by diffusion between the atoms, and, from the extreme minuteness of the mean radiation-distance, is concentrated to an enormous intensity. The æther, thus put into most vehement vibration, exerts at the same time, upon the atoms floating in it, through the relative differences of radiation (*i. e.* through the difference of its elasticity-forces on the average existing on opposite sides of the opaque atoms irradiating one another, and changing with their positions), proportionally intense motive forces, by which they are alternately retarded and accelerated according to their momentary positions ; and consequently their *vires vivæ* are to a corresponding amount expended on the production of æther-undulations, by the consumption of which they are again replaced.

According to Pouillet's determination of the intensity of the solar radiation, it is calculated that the mean square of the velocity of the æther for direct sunlight is to that of the æther in water at 0° C. approximately as 1 : 273,000,000,000 ; and accordingly it appears to the author possible that the atoms of a body reciprocally exert very intense accelerative forces (according to the hypothesis, in the sense of attractions) by the heat-rays which they emit to one another ; while the corresponding force which any available source of heat exerts upon a body irradiated by it is under all circumstances immeasurably little.—*Kaiserliche Akademie der Wissenschaften in Wien, mathematisch-naturwissenschaftliche Classe*, April 4, 1878.

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XII. *On the Figure of the Earth.*

By Colonel A. R. Clarke, *C.B., F.R.S.**

THE fraction $\frac{1}{300}$, which, in round numbers, is taken to express the ellipticity of the earth, has apparently a tendency, as far as it is deduced from the measurement of terrestrial arcs, to increase as the data of the problem are added to. The $\frac{1}{298}$, obtained by Airy and Bessel from the very imperfect data of forty years back, was replaced, on the completion of the Russian and English arcs in 1858, by $\frac{1}{294}$; and the geodetic work recently completed in India indicates a further increase of the fraction, and so an assimilation to that obtained from pendulum observations. The data of the Indian arc of 21°, as used in 1858, were vitiated by a serious uncertainty as to the unit of length used by Colonel Lambton in the measurement of the southern half of that arc. It appears from the Annual Reports of Colonel Walker, C.B., F.R.S., Surveyor-General of India, who has been for many years Superintendent of the Great Trigonometrical Survey of India (reports which are replete with scientific interest), that this southern portion of "the Great Arc," as Colonel Everest delighted to call it, has been completely remeasured and the latitudes of a great number of stations in it determined. A complete meridian chain of triangles has also been carried from Mangalore on the west coast, in latitude 12° 52′ and longitude 75° E., to a point in latitude 32°. As this triangulation is rigidly connected with the arc from Cape Comorin to Kaliana, in 78° E. longitude, it may be considered that the Indian Arc is now 24° in length.

* Communicated by the Author.

Colonel Walker's last Report contains the details of eleven determinations of difference of longitude by electro-telegraphy, with the corresponding geodetic differences. The differences of longitude are between Mangalore and Bombay on the west coast, Vizagapatam and Madras on the east coast, and Hydrabad, Bangalore, and Bellary in the interior. Bellary holds a somewhat central position in the polygon formed by the other points; Mangalore and Madras are very nearly in the same latitude; and Bangalore is midway between them. These differences of longitude have been determined with every refinement of modern science, and, taking into account the uncertainty of local attraction, may be considered little, if at all, inferior to latitude-determinations. "When the operations were commenced," says Colonel Walker, "I determined that they should be carried on with great caution and in such a manner as to be self-verificatory, in order that some more satisfactory estimate might be formed of the magnitudes of the errors to which they are liable than would be afforded by the theoretical probable errors of the observations. . . . . The simplest arrangement appeared to be to select three trigonometrical stations A, B, C, at nearly equal distances apart on a telegraphic line forming a circuit, and, after having measured the longitudinal arcs corresponding to A B and B C, to measure A C independently as a check on the other two arcs."

The eleven determinations of difference of longitude between the seven points named above give thus five equations of condition among themselves, which enable us to assign a system of minimum corrections to the several determinations. The following Table contains the observed differences, together with the computed corrections, half-weight being given to the first two determinations, which were the earliest made, and which are affected with some slight defects, then undiscovered, in one of the transit-instruments:—

Year.	Arc.	Electro-telegraphic difference of longitude.	Corrections.
		° ′ ″	″
1872–73.	Madras—Bangalore.	2 39 45·63	+2·010
,,	Bangalore—Mangalore.	2 44 11·54	+1·690
1875–76.	Hydrabad—Bombay.	5 42 12·74	−0·452
,,	Bellary—Bombay.	4 6 44·39	−0·393
,,	Hydrabad—Bellary.	1 35 28·25	+0·040
,,	Madras—Hydrabad.	1 43 40·38	−0·412
,,	Madras—Bellary.	3 19 8·45	−0·192
,,	Bangalore—Bellary.	0 39 20·46	+0·160
1876–77.	Vizagapatam—Madras.	3 2 26·78	+0·401
,,	Vizagapatam—Bellary.	6 21 35·84	−0·401
,,	Mangalore—Bombay.	2 1 50·54	+0·845

The following Table contains in the second column the geodetic longitudes as given by Colonel Walker, computed on Everest's elements; viz. equatorial semiaxis $a'=20922932$, polar semiaxis $c'=20853375$:—

Names.	Geodetic longitudes on Everest's spheroid.	Geodetic longitudes on undetermined spheroid.
Vizagapatam	83° 19′ 47″·00	$43''\cdot05-2\cdot320\,u-2\cdot401\,v$
Hydrabad	78 33 38·50	$37\cdot51-0\cdot580\,u-0\cdot600\,v$
Bombay	72 51 16·23	$18\cdot80+1\cdot511\,u+1\cdot563\,v$
Mangalore	74 53 10·18	$11\cdot44+0\cdot742\,u+0\cdot768\,v$
Bangalore	77 37 27·72	$27\cdot32-0\cdot234\,u-0\cdot242\,v$
Madras	80 17 21·87	$19\cdot85-1\cdot184\,u-1\cdot226\,v$
Bellary	76 58 6·97	

The third column contains (omitting degrees and minutes) the same longitudes on the supposition that the elements are

$$c=20855500\left(1+\frac{u}{10000}\right),$$

$$\frac{a-c}{a+c}=n=\frac{1}{590}+v\sin 10'',$$

which I take for the undetermined elements of the spheroid most nearly representing the mean figure of the earth. The terms in u and v added to the longitudes in the above Table are thus obtained:—Let A be the central point Bellary, B one of the other stations, Q the point in which the normal at A meets the axis of revolution; let θ be the angle subtended at Q by the curve distance A B, this curve being the intersection of the spheroid with the vertical plane at A passing through B; then, if A B$=s$, and ϕ_1 be the latitude of A, and α the azimuth of B at A,

$$\theta=\frac{s}{c}\,\frac{1-n}{(1+n)^2}(1+2n\cos 2\phi_1+n^2)^{\frac{1}{2}}(1+\tfrac{2}{3}n\theta^2\cos^2\phi_1\cos^2\alpha).$$

Thus for any variations of n and c a determinate variation arises for θ which may be expressed in the above u and v.

Again, the variation of θ gives rise to a variation of ω, the longitude of B computed from A, viz. $\delta\omega=\sin \mathrm{B}\sec\psi\,.\,\delta\theta$, where B is the azimuth of the curve A B at B, and ψ is the inclination of the line Q B to the equator.

Let us suppose the easterly component of the local attraction at A is y_1; then, longitudes being measured positively towards

the east, the observed or astronomical longitude of A must receive the correction $y_1 \sec \phi_1$; so that of B receives a corresponding correction $y_2 \sec \phi_2$; and the difference of longitude as observed (*i. e.* of B east of A) must receive the correction $y_2 \sec \phi_2 - y_1 \sec \phi_1$. The astronomical difference of longitude thus corrected is to be equated to the corresponding difference of geodetic longitudes as expressed in terms of u and v; then multiplying by $\cos \phi_2$, we get an equation of the form

$$y_2 = y_1 \sec \phi_1 \cos \phi_2 + \alpha u + \beta v + \gamma.$$

The above data afford six such equations. Properly speaking, as a direct check upon these equations, we should add to them the six equations of a similar character which would result from a comparison of the observed directions of the meridian at the seven stations we are considering. I have not, however, the quantities for forming these equations.

Besides the data contained in his last annual report, Colonel Walker has kindly given me provisional results for his great arcs (or arc, for we may consider them as one)—not final results, but yet not likely to be materially altered. The Indian Triangulation contains a vast number of astronomical stations; but in the problem of the figure of the earth it is not desirable that the latitude-points in one of the arcs should be very much more numerous than in the others. The Russian arc of 25° has thirteen astronomical stations; the English has thirty-four; but only fifteen are used in this investigation, this number including those of the French arc: the length of the conjoined English and French arcs is 22°.

Taking fourteen evenly distributed latitudes in the Indian arc, they require the corrections shown in the following Table (the column on the left gives the approximate latitude of each station):—

32° 2′	$-4{\cdot}14-8{\cdot}562\,u+5{\cdot}102v+0{\cdot}997\,x$
30 22	$-0{\cdot}25-7{\cdot}969\,u+4{\cdot}988v+0{\cdot}998\,x$
29 30	$+3{\cdot}37-7{\cdot}662\,u+4{\cdot}912v+0{\cdot}998\,x$
27 55	$-1{\cdot}98-7{\cdot}092\,u+4{\cdot}742v+0{\cdot}998\,x$
24 25	$+2{\cdot}22-6{\cdot}312\,u+4{\cdot}446v+0{\cdot}998\,x$
24 7	$-0{\cdot}93-3{\cdot}725\,u+4{\cdot}179v+0{\cdot}999\,x$
22 1	$-2{\cdot}17-4{\cdot}970\,u+3{\cdot}783v+0{\cdot}999\,x$
20 44	$+5{\cdot}51-4{\cdot}511\,u+3{\cdot}514v+0{\cdot}999\,x$
18 3	$+2{\cdot}65-3{\cdot}545\,u+2{\cdot}886v+0{\cdot}999\,x$
16 10	$+6{\cdot}09-2{\cdot}865\,u+2{\cdot}397v+0{\cdot}999\,x$
14 55	$-1{\cdot}75-2{\cdot}418\,u+2{\cdot}057v+1{\cdot}000\,x$
12 59	$+5{\cdot}18-1{\cdot}726\,u+1{\cdot}502v+1{\cdot}000\,x$
10 59	$+0{\cdot}43-1{\cdot}005\,u+0{\cdot}893v+1{\cdot}000\,x$
8 12	$0{\cdot}00\quad 0{\cdot}000\,u\quad 0{\cdot}000v+1{\cdot}000\,x$

It is interesting to consider the influence of each of the three great arcs in determining the semiaxes of the earth. The northern ten degrees from 60° to 70° of the Russian arc determine $\frac{3}{2}a-\frac{1}{2}c$; the ten degrees in England from 50° to 60° determine a; the ten degrees in France from 40° to 50° determine $\frac{1}{2}a+\frac{1}{2}c$; the ten degrees in India from 10° to 20° determine $-\frac{11}{14}a+\frac{2}{14}\frac{5}{}c$. Or, more precisely, taking the arcs in combination, suppose each arc to have six astronomical stations, equidistant, 5° apart in the Russian and 4° apart in the two other arcs; and let these arcs be combined by the method of least squares to determine the mean figure of the earth. Let $\theta_1 \dots \theta_6$ be the latitudes of the stations in the Russian arc, numbered from north to south; $\phi_1 \dots \phi_6$ those of the Anglo-French; $\psi_1 \dots \psi_6$ those of the Indian; then, supposing these expressed in seconds, a involves, in feet,

Russian.	Anglo-French.	Indian.
$-117{\cdot}6\,\theta_1$	$-76{\cdot}2\,\phi_1$	$-5{\cdot}4\,\psi_1$
$-\ 63{\cdot}7\,\theta_2$	$-40{\cdot}7\,\phi_2$	$+0{\cdot}3\,\psi_2$
$-\ 14{\cdot}5\,\theta_3$	$-\ 8{\cdot}8\,\phi_3$	$+3{\cdot}0\,\psi_3$
$+\ 29{\cdot}3\,\theta_4$	$+19{\cdot}1\,\phi_4$	$+3{\cdot}2\,\psi_4$
$+\ 67{\cdot}3\,\theta_5$	$+43{\cdot}2\,\phi_5$	$+1{\cdot}2\,\psi_5$
$+\ 99{\cdot}3\,\theta_6$	$+63{\cdot}4\,\phi_6$	$-2{\cdot}3\,\psi_6$

and c involves

Russian.	Anglo-French.	Indian.
$-26{\cdot}5\,\theta_1$	$-39{\cdot}6\,\phi_1$	$-112{\cdot}5\,\psi_1$
$-23{\cdot}0\,\theta_2$	$-28{\cdot}9\,\phi_2$	$-\ 71{\cdot}1\,\psi_2$
$-14{\cdot}5\,\theta_3$	$-14{\cdot}6\,\phi_3$	$-\ 26{\cdot}7\,\psi_3$
$-\ 0{\cdot}6\,\theta_4$	$+\ 3{\cdot}9\,\phi_4$	$+\ 20{\cdot}3\,\psi_4$
$+19{\cdot}3\,\theta_5$	$+26{\cdot}3\,\phi_5$	$+\ 69{\cdot}5\,\psi_5$
$+45{\cdot}4\,\theta_6$	$+52{\cdot}8\,\phi_6$	$+120{\cdot}3\,\psi_6$

From which we see at a glance the effect that would result from an alteration of any one of the latitudes.

It seems unnecessary to give here the expressions for the corrections to the stations of the English, the Russian, Cape-of-Good-Hope, and Peruvian arcs, which are to be found in a paper on the Figure of the Earth, in the Memoirs of the Royal Astronomical Society for 1860, pp. 34, 35. It is only necessary to remark that the sign of u in those expressions is to be changed, and that I have now added three points to the Anglo-French arc as there used. Making now the sum of the squares of the corrections, or local attractions, at the forty-nine latitude stations and the seven longitude stations

a minimum, the resulting equations in u and v are

$$0 = +56{\cdot}6615 + 301{\cdot}7624\,u + 126{\cdot}9252\,v,$$
$$0 = -16{\cdot}9677 + 126{\cdot}9252\,u + 221{\cdot}4307\,v;$$
$$\therefore\ u = -0{\cdot}2899\,;\quad v = +0{\cdot}2428.$$

From these we have, in feet of the standard yard,

$$\left.\begin{aligned} a &= 20926202, \\ c &= 20854895, \\ \frac{c}{a} &= \frac{292{\cdot}465}{293{\cdot}465}. \end{aligned}\right\} \quad \ldots\ldots\ldots \quad (\mathrm{E})$$

And this is the spheroid most nearly representing the mean figure of the earth.

But the Indian observations are not well represented by this figure. The southern station of the arc requires a large negative correction of $-3''{\cdot}14$, and the northern station a still larger negative correction of $-3''{\cdot}55$. Among the longitude stations, there is left at Bombay a westerly deflection of $4''{\cdot}05$, and at Madras an easterly deflection of $4''{\cdot}50$. The longitudes, in fact, require a larger value of a and a larger value of the ellipticity; while the form of the meridian-arc requires a smaller equatorial radius and a smaller ellipticity. In other words, so far as the observations we have at present to consider indicate, the surface of India does not seem to belong to a spheroid of revolution: if it does, we must admit large deflections towards the sea at Cape Comorin, at Bombay, and at Madras.

But we may obtain more strictly the form of the Indian arc from the sixty-six latitude stations it contains. Not to confine the arc to an elliptic form, let it be such that its radius of curvature in latitude ϕ is expressed by the equation

$$\rho = \mathrm{A}' + 2\mathrm{B}'\cos 2\phi + 2\mathrm{C}'\cos 4\phi,$$

a curve which includes the ellipse as a particular case. In order to determine A, B, C, we must apply symbolical corrections to the observed latitudes, and make the sum of the squares of these corrections a minimum. As the result of a very long calculation, the actual equation is found to be

$$\rho = 20932184{\cdot}1 - 167963{\cdot}6\cos 2\phi + 28153{\cdot}2\cos 4\phi. \quad . \quad (\mathrm{E}')$$

The correction to the latitude of the southern point is $+1''{\cdot}61$, and to the northern $-0''{\cdot}81$; and, generally, the residual corrections or apparent local attractions are free from any appearance of law, so that the above equation may be taken as very closely representing the form of the sea-level along the meridian of India. The geodetic operations give us the *form* of the

curve in the shape of its intrinsic equation; and the absolute direction of the curve with reference to the polar axis is given by observing that, at its southern extremity, the actual direction of the surface of the sea makes an angle of 1″·61 with the curve. Now, the observed latitude of the southern point being 8° 12′ 10″·44, the direction of the normal to our curve (E′) at the same point makes the angle 8° 12′ 12″·05 with the plane of the equator, which determines the curve as to its absolute direction. So also, on referring the Indian meridian to the ellipse (E), determined above as representing the mean figure of the earth, this ellipse at the southern point of the Indian arc has its normal inclined to the equator at an angle equal to observed latitude $-3''\cdot14$, or 8° 12′ 7″·30. We can now trace the difference of the forms of the curves (E, E′) by making them coincide at the southern point of the arc. The selection for this purpose of the southern point is quite arbitrary; any other station would have done equally well. Multiply the expression for ρ, the radius of curvature, by $-\sin\phi\, d\phi$ and then by $\cos\phi\, d\phi$, and integrate; thus we get the following values of the coordinates of the curve (E′) in the meridian plane, parallel and perpendicular to the equator:—

$$\left.\begin{aligned} x' &= (A'-B')\cos\phi + \tfrac{1}{3}(B'-C')\cos 3\phi + \tfrac{1}{5}C'\cos 5\phi + H, \\ y' &= (A'+B')\sin\phi + \tfrac{1}{3}(B'+C')\sin 3\phi + \tfrac{1}{5}C'\sin 5\phi + K, \end{aligned}\right\} \quad (E')$$

when H and K are disposable constants. The corresponding coordinates of the ellipse (E) may also be written in the form

$$\left.\begin{aligned} x &= (A-B)\cos\phi + \tfrac{1}{3}(B-C)\cos 3\phi + \tfrac{1}{5}C\cos 5\phi, \\ y &= (A+B)\sin\phi + \tfrac{1}{3}(B+C)\sin 3\phi + \tfrac{1}{5}C'\sin 5\phi. \end{aligned}\right\} \quad (E)$$

The values of H and K are now to be determined by putting $\phi = 8^\circ\ 12'\ 12''\cdot05$ in the expressions for x' and y', and $\phi = 8^\circ\ 12'\ 7''\cdot30$ in those for x and y; then putting $x = x'$, $y = y'$.

The normal distance between the curves (E, E′) in latitude ϕ is $\zeta = (x'-x)\cos\phi + (y'-y)\sin\phi$: this expresses the distance by which a point in (E′) is further from the centre of the earth than the corresponding point of (E). Put $A'-A=E$, $B'-B=F$, $C'-C=G$; then

$$\zeta = E - \tfrac{2}{3}F\cos 2\phi - \tfrac{2}{15}G\cos 4\phi + H\cos\phi + K\sin\phi.$$

The following Table shows, according to this formula, the departure of the curve best representing the Indian meridian from that best representing the earth as a whole. I add also similar quantities for the Russian and Anglo-French arcs; the only difference is that, in the case of these last arcs, the local

curve is simply that elliptic curve which best represents the observations.

Indian.		Anglo-French.		Russian.	
Lat.	ζ.	Lat.	ζ.	Lat.	ζ.
	ft.		ft.		ft.
°		°		°	
10	−11·8	40	+ 8·1	48	−2·7
12	−18·5	42	+15·7	50	−3·7
14	−19·6	44	+18·9	52	−4·0
16	−16·7	46	+18·8	54	−3·7
18	−11·1	48	+16·1	56	−2·9
20	− 4·3	50	+11·8	58	−1·8
22	+ 2·1	52	+ 6·8	60	−0·5
24	+ 6·9	54	+ 1·9	62	+0·8
26	+ 9·3	56	− 1·8	64	+2·0
28	+ 8·3	58	− 3·6	66	+3·1
30	+ 3·8	60	− 2·7	68	+3·8
32	− 4·2			70	+4·1

Here we see the local form of the meridian sea-level in India with reference to the mean figure of the earth. Supposing that there is no disturbance of the sea-level at Cape Comorin, then from that point northwards a depression sets in, attaining a maximum of nearly 20 feet at about 14° latitude; thence it diminishes, disappearing at about 21°. An elevation then commences, attaining at 26° about nine feet; then this elevation diminishes, and becomes a small depression at 32°. This deformation may or may not be due to Himalayan attraction; at any rate we have here an indication that that vast tableland does not produce the disturbance that might *à priori* have been anticipated. This is in accordance with the fact that there is an attraction *seaward* at Mangalore and Madras, and slightly also at Bombay: and I think we have here a corroboration of Archdeacon Pratt's theory, that where the crust of the earth is thickest there it is least dense; and where thinnest, as in ocean-beds, there it is most dense.

The Anglo-French arc shows a deformation nearly as great as the Indian—though, after all, the linear magnitude in either case is certainly as small as could be expected. One cannot help remarking here, that the remeasurement of the French meridian-arc, with all modern refinements of observation and calculation, with a considerable increase in the number of latitude stations, would be a vast service to science.

With the elements of the earth's spheroidal figure at which we have arrived above (E) the following results are obtained. The radii of curvature in and perpendicular to the meridian in latitude ϕ being ρ, ρ', their values in standard feet are,

$$\rho = 20890564 - 106960\cos 2\phi + 228\cos 4\phi,$$
$$\rho' = 20961932 - 35775\cos 2\phi + 46\cos 4\phi.$$

The lengths of one degree in and perpendicular to the meridian, viz. δ, δ', are

$$\delta = 364609{\cdot}12 - 1866{\cdot}72\cos 2\phi + 3{\cdot}98\cos 4\phi,$$
$$\delta' = 365854{\cdot}72 - 624{\cdot}40\cos 2\phi + 0{\cdot}80\cos 4\phi.$$

Also the following:—

$$\log\frac{1}{\rho\sin 1''} = 7{\cdot}994477820 + {\cdot}002223606\cos 2\phi - {\cdot}000001897\cos 4\phi,$$

$$\log\frac{1}{\rho'\sin 1''} = 7{\cdot}992994150 + {\cdot}000741202\cos 2\phi - {\cdot}000000632\cos 4\phi.$$

Having seen that the surface of India cannot be represented by a spheroid of revolution, it is necessary now to inquire what ellipsoid best represents all the observations as the figure of the earth. On this hypothesis, the equator being no longer a circle, the ellipticity of a meridian is not a constant, but is a function of the longitude—say l, from Greenwich. We have consequently to replace our previous v by $v + w\cos 2l + z\sin 2l$; and the longitude l' of the greater semiaxis of the equator will be given by the equation $w\sin 2l' - z\cos 2l' = 0$. But this substitution cannot be made in the longitude-equations—they no longer hold good, having been formed on the distinct supposition of the earth being a surface of revolution, and they must now be put aside. If the earth should be found to be really ellipsoidal, this circumstance will involve a considerable increase of the labours of the geodetic computer. The "meridian" on an ellipsoid is somewhat vague. If it be taken as the locus of points of constant longitude ω, its equation in combination with that of the ellipsoid is

$$b^2x\sin\omega - a^2y\cos\omega = 0. \quad . \quad . \quad . \quad . \quad (1)$$

But it may also be defined as a line on the ellipsoid whose direction is always north and south. Suppose that a point on the surface of the ellipsoid $\frac{x^2}{a^2} + \frac{y^2}{b^2} + \frac{z^2}{c^2} = 1$ moves always towards a given fixed point $x'y'z'$, and let it be required to determine the nature of the curve traced by the moving point. Two consecutive points on the curve having coordinates x, y, z, $x+dx, y+dy, z+dz$ give the condition

$$\frac{x}{a^2}dx + -\frac{y}{b^2}dy + \frac{z}{c^2}dz = 0. \quad . \quad . \quad . \quad . \quad (2)$$

The equation of a plane passing through x, y, z and x', y', z' is

$$\mathrm{A}(x'-x) + \mathrm{B}(y'-y) + \mathrm{C}(z'-z) = 0.$$

This plane is to contain the normal at x, y, z, and the point $x+dx, y+dy, z+dz$, which conditions give two other equations in A, B, C; and eliminating these symbols we have the differential equation of the required curve expressed by the determinant

$$\begin{vmatrix} x'-x, & y'-y, & z'-z, \\ dx, & dy, & dz, \\ \frac{x}{a^2}, & \frac{y}{b^2}, & \frac{z}{c^2}. \end{vmatrix}$$

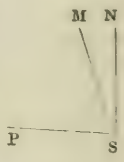
A north-and-south line is a particular case of this curve, viz. when $x'=0$, $y'=0$, $z'=\infty$; then the equation becomes

$$a^2 y\,dx - b^2 x\,dy = 0, \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

of which the integral is

$$x^{a^2} = c y^{b^2}.$$

This is not a plane curve; and at each point its direction makes a definite angle with the meridian as expressed by (1). Let S be any point on the surface of the ellipsoid, say in that octant where x, y, z are all positive; let M be a point indefinitely near S on the same meridian (1), N a point on the north line (3), P a point on the parallel of latitude through S, of which, ϕ being the latitude, the equation is



$$\frac{x^2}{a^4} + \frac{y^2}{b^4} - \frac{z^2}{c^4}\cot^2\phi = 0. \quad . \quad . \quad . \quad . \quad . \quad (4)$$

The differential equation of the meridian is

$$-b^2 \sin\omega\,dx + a^2 \cos\omega\,dy = 0. \quad . \quad . \quad . \quad . \quad (5)$$

And if from this equation, with (2), we determine the ratios of dx, dy, dz, they are found to be proportional to

$$-a^2 z\cos\omega \;:\; -b^2 z\sin\omega \;:\; c^2(x\cos\omega + y\sin\omega) \quad . \quad (6)$$

And these are proportional to the direction-cosines of SM. So also, getting the ratios of dx, dy, dz from (2) and (3), we find the direction-cosines of SN to be proportional to

$$-b^2 xz \;:\; -a^2 yz \;:\; c^2\left(\frac{a^2}{b^2}y^2 + \frac{b^2}{a^2}x^2\right), \quad . \quad . \quad . \quad . \quad (7)$$

Similarly for the direction-cosines of SP; they are as

$$-a^2 yz\left(\frac{1}{b^2} + \frac{\cot^2\phi}{c^2}\right) : b^2 xz\left(\frac{1}{a^2} + \frac{\cot^2\phi}{c^2}\right) : c^2 xy\left(\frac{1}{b^2} - \frac{1}{a^2}\right). \quad (8)$$

These enable us to determine the angles between the lines SM,

SN, and SP. Let the semiaxes of the equator be expressed by the relations

$$a^2=k^2(1+i)\,;\quad b^2=k^2(1-i),$$

where i is a very small quantity whose square is to be neglected. Then the coordinates x, y, z of any point, as S, are proportional to

$$(1+i)\cos\omega : (1-i)\sin\omega : \frac{c^2}{k^2}\tan\phi.$$

Substitute these in (6), (7), (8), and we get finally the following results for the angles between the lines in question :—

$$\mathrm{MSP}=\frac{\pi}{2}-i\sin\phi\sin 2\omega\left(1+\frac{c^2}{c^2\sin^2\phi+k^2\cos^2\phi}\right),$$

$$\mathrm{NSP}=\frac{\pi}{2}-i\frac{c^2\sin\phi\sin 2\omega}{c^2\sin^2\phi+k^2\cos^2\phi},$$

$$\mathrm{MSN}=i\sin\phi\sin 2\omega.$$

In the figure of the earth, as determined in the paper in the 'Memoirs of the Royal Astronomical Society' for 1860, there is a difference of a mile between the greatest and least radii of the equator. Although this seems but a small departure from the form of a circle, yet $i=52''{\cdot}33$ (in parts of radius unity), and the angles expressed above become somewhat large quantities. Supposing S to be on a meridian midway between the greatest and least radii of the equator, the angle between the "meridian" and the "north line" is $52''{\cdot}33\sin\phi$; and the defect of MSP from a right angle is about double this quantity. So large an angle as this should be detected by firstrate geodetic observations, though it would require a somewhat long measurement of meridian and parallel. It is to be remembered that, SM, SN being directed towards the north, and SP towards the minor axis of the equator, SM lies between SP and SN.

And in an ellipsoidal earth the direction of the principal sections of the surface (that is, of maximum and minimum curvature) are no longer coincident with meridians, north lines, or parallels. Supposing that S is not in a very high latitude, one of the lines of curvature, as SR through S, will lie somewhere in the direction of SP, and the second line of curvature will be perpendicular to SR. It may be shown that the angle

$$\mathrm{RSN}=\frac{\pi}{2}-i\sin 2\omega\sin\phi\sec^2\phi\,\frac{c^2}{k^2-c^2},$$

an expression which does not hold in high latitudes; for in the vicinity of the umbilics, the lines of curvature are approximately confocal conics having the umbilics as foci. The defect

of RSN from a right angle might, with the value of i we have been supposing, amount to some degrees without going to any high latitudes.

It appears, then, that it would not do to take the longitude-equations which we have used for the determination of a spheroidal figure for the earth also for the determination of an ellipsoidal figure. The only thing that can be done under the circumstances is to take simply the longitude-arc between Bombay and Vizagapatam, as these points are nearly in the same latitude, and to reduce it according to the expression for the length of an arc of parallel on the surface of an ellipsoid, given in the before-mentioned paper on the Figure of the Earth, page 43.

Then, with fifty-one equations I get the following:—

$$u = -0{\cdot}4903\,;$$
$$v = +0{\cdot}2842\,;$$
$$w = +0{\cdot}3599\,;$$
$$z = -0{\cdot}1067.$$

From these quantities the following values finally result:—

$$a = 20926629\,;$$
$$b = 20925105\,;$$
$$c = 20854477.$$

If by the word "ellipticity" of an ellipse we mean the ratio of the difference of the semiaxes to half the sum of the same, the ellipticities of the two principal meridians of the earth are

$$\frac{1}{289{\cdot}54}, \quad \frac{1}{295{\cdot}77}.$$

The longitude of the greater axis of the equator is 8° 15′ west of Greenwich—a meridian passing through Ireland and Portugal and cutting off a portion of the north-west corner of Africa; in the opposite hemisphere this meridian cuts off the north-eastern corner of Asia and passes through the southern island of New Zealand. The meridian containing the smaller diameter of the equator passes through Ceylon on the one side of the earth and bisects North America on the other. This position of the axis, brought out by a very lengthened calculation, certainly agrees very remarkably with the physical features of the globe—the distribution of land and water on its surface. On the ellipsoidal theory of the earth's figure, small as is the difference between the two diameters of the equator, only 3000 feet, the Indian longitudes are better represented than on the spheroidal; but there is still left at Madras and Mangalore an attraction or disturbance of the plumb-line seawards.

As to the relative evidence for the two figures presented in this paper, the sum of the squares of the residual corrections to the astronomical observations is, of course, less in the ellipsoid than in the spheroid ; but the difference is certainly small. The radius of curvature perpendicular to the meridian in India, in latitude 15° say, is, on the spheroid, 20930972 feet, whereas on the ellipsoid it is 20932877 ; and this last is distinctly more in harmony with the Indian Longitude Observations.

Ordnance Survey Office, Southampton,
June 15, 1878.

XIII. *On Telephony.* *By* W. Siemens*.

THE surprising performances of the telephones of Bell and Edison rightly claim in a high degree the interest of natural philosophers. The solution (facilitated by it) of the problem of the conveyance of tones and the sounds of speech to distant places promises to give mankind a new means of intercourse and culture which will essentially affect their social relations and also render substantial service to science ; and hence it seems fitting that the Academy should draw these exceedingly promising discoveries into the sphere of its contemplations.

The possibility of reproducing mechanically not merely tones, but also noises and spoken sounds, at great distances is given theoretically by Helmholtz's path-opening investigations, which elucidated the essential nature of shades of tone and the sounds of speech.

If, as he has demonstrated, noises and sounds are only distinguished from pure tones by the fact that the latter consist of simple, the former of a plurality of series of undulations, superposed to one another, of the sonorific medium, and if the noises of speech (*Sprachgeräusche*) may be conceived as irregular vibrations with which the vocal sounds begin or end, then it is also possible to reproduce mechanically a certain succession of such vibrations at distant localities. Indeed practical life has in this, as is frequently the case, outrun science. The hitherto too little regarded so-called "speaking telegraph," consisting of two membranes stretched by a strong and at the same time extremely light thread or fine wire which is fastened to their centres, effects a perfectly distinct transmission of speech to a distance of several hundred metres. The threads or wires can

* Translated from the *Monatsbericht der königlich preussischen Akademie der Wissenschaften zu Berlin*, January 1878, pp. 38–53.

be supported at any number of points by elastic threads of a few inches length, and also, with similar elastic fastenings at the angles, from any number of angles, without the apparatus losing the capability of conveying with perfect distinctness and correctness completely toneless whispered speech—a performance which previously no electric telephone could accomplish. Although this "speaking telegraph," or, more correctly, "thread telephone," possesses no practical value (since its working is still limited to short distances and is interrupted by wind and rain), yet it is most deserving of notice, because it proves that stretched membranes are fitted to take up, almost completely, all the air-vibrations by which they are struck, and to reproduce in another place all speech-sounds and noises when mechanically put into similar vibrations.

Reis, as is well known, was the first to endeavour to operate the conveyance of tones by electric currents instead of a stretched thread. He made use of the vibrations of a membrane exposed to sound-waves to produce closing contacts of a galvanic series. The current-waves hereby generated traversed, at the other end of the conduction, the coil of an electromagnet, which, provided with a suitable resonance-arrangement, again produced approximately the same tones by which the membrane, struck by the sound-waves, had been set vibrating. This could only be done very imperfectly, since the contact-arrangements only became effective with the greater vibrations of the membrane, and could only imperfectly render even these.

Bell appears first to have had the happy thought to let the vibrating membrane itself call forth the currents serving for the transmission of its vibrations—making it of soft iron, and placing its centre opposite and very near to the end of a steel magnet wound round with insulated wire. By the vibrations of the membrane the attraction between the plate and the magnet, and therewith the magnetic potential of the wire-enveloped end of the bar-magnet, were alternately augmented and diminished; by this, in the wire of the coil and in the conduction, currents were produced which, with the minuteness of the vibrations of the plate, generated electrical sine vibrations corresponding to the vibrations of the mass of air, which were thus in a condition to call forth again membrane- and air-vibrations in a similar apparatus at the other extremity of the conduction. The result was unaffected by the circumstance that, as Du Bois-Reymond* has pointed out, in the receiving membrane the phases and ratios of amplitude of the partial tones are different from those in the emitting membrane.

* *Archiv für Physiologie*, 1877, pp. 573, 582.

An essentially different path was struck out by Edison (as it appears, simultaneously with Bell). He uses a galvanic series, which sends a constant current through the conduction. At the sending end a layer of powdered graphite, which is gently pressed between two metal plates insulated one from the other, is inserted in the circuit. The upper plate is fastened to the vibrating membrane, and presses the graphite powder more or less together in correspondence with the air-vibrations. By this the resistance of the graphite to conduction is correspondingly varied, and thereby sinusoid variations, equivalent to the air-vibrations, are produced in the intensity of the current passing through the conducting line. As receiving-apparatus, Edison uses no membrane, but another and quite peculiar contrivance. It is based on the experience that the friction between a piece of metal and a paper band saturated with a conducting fluid and pressed against the metal is diminished when a current passes through the paper to the metal. I have verified this remarkable phenomenon for the case in which the direction of the current is such that hydrogen is separated at the metal plate, or when the metal is not oxidizable. Hence the lessening of the coefficient of friction by the current evidently proceeds from electrolytically generated gases deposited on the plate of metal. Surprising, however, remains the almost instantaneous rapidity with which the effect takes places even with very feeble currents.

Now Edison attaches the metal plate, pressed against the moist paper, to a sounding-board, and draws the moist paper, which is carried over a roller, under the metal piece by continual rotation of the roller. If now the metal piece and the roller (also of metal) be inserted in the galvanic circuit, the variations produced in the current by the greater or less pressure of the graphite powder effect equivalent variations of the friction-coefficient between the metal plate attached to the sounding-board and the paper, whereby the former is put into corresponding vibrations, which are communicated to the sounding-board, and through this to the air.

Edison's telephone is very remarkable on account of the novelty of the expedients employed in it; but it is obviously not yet complete for practical use; while Bell's telephone has, in its remarkably simple form, been widely spread in a short time, especially in Germany; and already much material of experience has been accumulated for judging of its usefulness. Its principal defect consists in the feebleness of the reproduced sounds, which, in order to be distinctly understood, require the sound-aperture to be pressed to the ear, and at the other end an immediate speaking into it. Per-

fect quiet is necessary around it, in order that the ear may not be dulled and disturbed by extraneous noises. A still graver obstacle to its practical employment consists in this—that it needs complete electrical calm. As the currents are extraordinarily feeble which are generated by the vibrating iron membrane and put the membrane of the other instrument into similar vibrations, therefore also very weak extraneous currents are sufficient to disturb the latter and bring to the ear confusing noises of other origin.

In order to procure fixed points for judging the intensity of the currents which are effective in the telephone, I placed one of Bell's telephones, the magnet-pole of which was wound round with 800 turns of copper wire 0·1 millim. thick and possessing a resistance of 110 mercury units, in a circuit containing a Daniell's cell, with a commutator, by which the direction of the current was reversed 200 times in a second.

Without an inserted resistance, these current-waves produced in the telephone an extremely inharmonious noise, audible at a long distance, and almost intolerable close to the ear. By the insertion of a resistance, this noise was diminished, but was still very loud after the insertion of 200,000 units. If 6 Daniells were inserted, the noise was still distinctly audible through ten million units of resistance. If 12 Daniells and twenty million units of resistance were inserted, the sound was decidedly more distinct than in the last preceding case. In like manner an increase in its intensity took place when thirty and fifty millions of units were inserted with 18 and 30 Daniells respectively. This corroborates Beetz's observation that electromagnetism, with equal current-intensity, is more quickly called forth in circuits of great resistance by correspondingly more intense electromotive forces, than in circuits with little resistance and proportionately less electromotive forces, because the countercurrents which arise in the windings of the electromagnet count for more in the latter case than in the former.

If in the circuit of the commutator the primary spiral of a small voltaic induction-coil was inserted, such as is ordinarily employed by physicians, while the telephone and resistance-scale were in the circuit of the secondary wire, with one Daniell element a loud-sounding noise was still obtained when fifty million mercury units were inserted; and this remained distinctly audible even when the secondary spiral was pushed back right to the end of the primary.

This sensitiveness of Bell's telephone to feeble currents renders it very useful as a galvanoscope, especially for the detection of feeble and rapidly changing currents, for which there

has hitherto been scarcely any other means of testing than the contractions of the leg of a frog. Also, in measuring resistances by the bridge method, the telephone may often be employed with advantage instead of the galvanometer in the branch wire of the bridge; but then it will be necessary to employ as resistances only straight wires stretched at a greater distance from each other, as otherwise perturbations would arise through induction.

This perfectly explains the extreme sensitiveness of the telephone to electrical disturbances in the conductor, which, indeed, almost entirely excludes its application to lines above ground if the same posts support wires which are used for telegraphic correspondence. Even when two neighbouring conducting wires on the same posts are employed to form the conduction-circuit, in which case the electrodynamic as well as the electrostatic induction proceeding from the other more distant wires is in great part compensated, still every current that passes through these wires is heard in the telephone as a loud cracking noise rendering the speech of the telephone quite unintelligible if it is frequently repeated.

Far worse still are these disturbances if the earth is used for closing the circuit. Even when special earth-plates are taken for the telephone-wire, or if gas- or water-pipes are made use of for the same purpose, every current is distinctly heard which is brought to earth through earth-plates in the vicinity. Since, in the spreading of a current in the ground, the electric potential diminishes with the cube of the distance from the point at which the current enters the earth, this also demonstrates the uncommon sensibility of the telephone to feeble currents.

For these reasons, with overland conducting-wires telephones can only be employed if special posts are appropriated to the support of the wires. Further, the earth's conduction can only be used in places where there are no telegraph-stations, or where the earth-plates used for telegraphing are at a good distance from those which serve for the telephone-conductions.

Notwithstanding this sensitiveness of Bell's telephone, it conveys but very imperfectly the sound-waves by which its membrane is struck to the corresponding membrane and the ear applied to it. When a loud-ticking watch was placed close to the sound-aperture of a very sensitive telephone constructed after Bell's plan, the ticking could not be heard in the other telephone, even when the watch actually touched the telephone-case. On the other hand, the above-mentioned thread telephone transmitted the ticking through a thread about 20

metres long very distinctly; it was still audible when the ear was withdrawn 8 centims. from the mouth of the hearing-tube. The ticking could be heard direct with about equal distinctness at the distance of 130 centims.; consequently the thread telephone conveyed about $\frac{1}{260}$ of the intensity of the sound. Since the electric telephone transmitted the softest speech intelligibly, it must be on account of the rapid and irregular vibrations which form the toneless, even if louder, noise of the ticking, that it cannot transmit the latter.

From a like cause a proper, perfectly toneless whisper cannot be understood through the electric telephone, while through the thread telephone it is distinctly intelligible to a distance of 20 metres. Just so electric telephones, which reproduce the softest speech distinctly, do not convey at all, or scarcely perceptibly, the loud but toneless clap of two pieces of iron or glass struck together.

It is remarkable that the electric telephone, in spite of this almost incapability of conveying the noises which consist of rapid and irregular vibrations, yet so truly renders the quality of musical tones and the sounds of speech that the voices of the speakers can be almost as well recognized through the telephone as direct from the speakers themselves. The voice, however, sounds somewhat fuller, which is to be ascribed to the circumstance that the tones are reproduced better and more powerfully than the noises of speech. Singing, too, sounds through the telephone, as a rule, softer and richer.

In order to gain a fixed point for the solution of the question what fraction of the force of the sound which strikes the membrane of the one telephone is given again by the other, I instituted some experiments with musical boxes. The smaller one, which gave short sharp tones, could be heard by good ears at 125 metres distance upon an open plain, while only isolated tones could be heard through the telephone when it was placed more than 0·2 of a metre from the musical box. In this instance, therefore, only about $\frac{1}{390000}$ of the sound was actually conveyed. A somewhat larger musical box, of not so high a pitch, and giving tones of longer duration, could not be heard in the open air much further than the smaller one; but the telephone at 1·2 metre distance caused the tune played to be recognized. This gives a conveyance of about $\frac{1}{10000}$ of the sound-intensity received by the telephone. Now, although the sounds of speech, as well as deeper and more sustained tones, are probably conveyed better than the melody of the musical boxes, it cannot be assumed that a Bell telephone conveys, on the average, more than $\frac{1}{10000}$ of the mass of sound by which it is struck, to the other telephone.

It follows from the above that Bell's telephone, notwithstanding its surprising performances, effects the conveyance of sound only in a very imperfect manner. That we can understand the speech of the telephone excited by currents so extraordinarily feeble, we owe to the extreme sensibility and great range of our organ of hearing, which enable it to bear the sound of a cannon at 5 metres distance, and yet to hear it at a distance of 50 kilometres, consequently to have the sensation of sound from air-vibrations within a range of from 1 to 100,000,000-fold intensity.

Accordingly the telephone needs, and is in a high degree capable of, improvement. Although it is not possible entirely to do away with loss of sound (which would be approximately accomplished if it could be effected that the vibrations of the second membrane should possess the same amplitude as those of the first), since in the repeated transformations of motions and forces there must always be a loss of *vis viva* by conversion into heat, yet the present disproportion is much too great. But by diminishing this loss, and thereby strengthening the arriving sound, we should secure that the hearing would need less exertion, and could distinctly perceive and distinguish the transmitted sounds at a greater distance from the instrument. Then also the perturbations produced by extraneous feeble electric currents would be felt less disturbing, because they would be covered by the more powerful arriving speech-sounds.

Hereby is also given the direction which must be taken for the improvement of Bell's telephone. In order to produce more intense currents, the membrane destined to receive the sound-waves must be sufficiently large and of such a constitution that the sound-waves striking its surface can impart to it a maximum of their *vis viva* ; while the membrane must be sufficiently movable for its vibrations not to be too small ; and the work expended for the production of the electric currents must be so much that the *vis viva* accumulated in the vibrations of the membrane will be consumed by it—or, in other words, so much as to make the membrane-vibrations aperiodic. An enlargement of Bell's iron sheet would be advantageous only within narrow limits, since larger and correspondingly thicker plates are apt to assume vibrations of their own, which diminish the distinctness of the transmitted sounds. It is also requisite that the magnetic attraction of the iron plate in Bell's telephone be not raised too high, as otherwise the plate is too much curved and stretched in one direction, which likewise detracts from the clearness.

I have tried, with considerable success, to strengthen the

attraction between the iron membrane and the wire-coiled magnet-pole without bringing the former out of its position of equilibrium, by bringing it between the poles of a powerful horse-shoe magnet.

The pole which was above the iron plate had the shape of a ring, the opening of which formed the sound-hole, while the lower pole of the horse-shoe supported the iron pin with a wire coil opposite to the centre of the sound-aperture. The membrane itself consisted of iron only in the middle, as far as it was opposite to the ring-shaped pole, while the other portion was made of sheet-brass, to which the iron was soldered. Through the action of the magnetic iron ring the middle of the iron plate became itself strongly magnetic ; consequently there was a very much strengthened attraction between it and the magnetic iron pin placed opposite to it, while the iron plate, attracted with equal force on both sides, remained, with the whole membrane, in the position of equilibrium, and could therefore vibrate freely towards both sides.

Another modification consisted in making both poles of the magnet ring-shaped, and providing them with short notched iron tubes wrapped round with spirals. There were now exactly opposite to the iron plate two ring-shaped magnet-poles of the same kind, while itself possessed the opposite polarity. This is the combination I often employ with good results in so-called polarized relays, in which the movable, powerfully magnetized iron tongue is situated between two oppositely magnetic poles of a magnet, at equal distance from each, of which the ends are provided with coils.

This arrangement has also been approved for telephonic call-signal apparatus. If a point in the rim of a steel bell, attached to one pole of a horse-shoe magnet, is between two iron pins furnished with coils, which form the other pole of the horse-shoe, a second bell, of the same pitch and with the same arrangement, repeats with surprising force the sound of every stroke made upon the other, if the coils of both are included in a conduction-circuit. The effect is the same with tuning-forks in unison.

Instead of *two* bells or tuning-forks, it is sufficient to insert only one in the telephone-circuit, if the question is only the conveyance of the sound of the bell as an alarm-signal. Telephones then give loud-sounding strokes of a bell.

If in this way the capabilities of the telephone can be considerably heightened, yet, in retaining Bell's iron membrane, we are restricted within rather narrow limits, both as regards the size of the membrane for receiving the sound and the strength of the effective magnetism, an excess of which ren-

ders the speech-sounds indistinct and accompanies them with a strange, unpleasant clang.

Hence, for the construction of larger telephones delivering much more powerful currents, I do not use any vibrating plate of iron, but I fix to the membrane that receives the sound-waves (which is made of non-magnetic material) a light coil of wire which waves freely in a ring-shaped strongly magnetic field. By the vibrations of the coil, intense currents alternating in direction are induced in it, which at the other extremity of the conduction set in similar vibrations either the coil of a similar instrument or the iron membrane of a Bell telephone.

As the breadth of a flat membrane cannot exceed rather narrow limits without confusing the speech-sounds transmitted, by the advice of Prof. Helmholtz I have given to the membrane the form of the tympanum of the ear. This form is obtained, according to Helmholtz, when a moist skin of parchment or a bladder is stretched over the rim of a ring, and then its centre gradually depressed to the desired depth by a screw or otherwise. The membrane will then retain this form after drying. If now a model be made after this form in metal, with its aid a membrane of sheet-brass, or, better, aluminium can be pressed so as to have the same form as the former. Membranes of this shape are especially suitable for the reception of sound-waves and for the transference of their *vis viva* to masses that are to be set vibrating (a purpose which they have to fulfil in the ear also), since their flexion results chiefly near the margin of the membrane—while in flat membranes it takes place more in the vicinity of the centre, and hence with these only those sound-waves which strike the middle of the plate come into full action. Such a telephone with a parchment membrane 20 centims. in diameter, a wire coil of 25 millims. diameter, 10 millims. height, and 5 millims. thickness, in a magnetic field of great intensity generated by a powerful electromagnet, transmits with perfect distinctness to a great number of smaller telephones every sound produced in any part of a room of moderate size; and the purity and clearness with which it transmits the sounds of speech and musical notes are remarkable—which may arise partly from the appropriate form of the membrane, and partly from the coil, on moving in the cylindrical magnetic field, generating more regular sinusoid currents than a vibrating iron plate. An apparatus in which such a wire coil is moved rapidly up and down by means of a winch with a long connecting-rod could be used with advantage for the generation of sine-currents of great intensity.

For giving back the sounds of speech the tympanic form of membrane is not so well suited. It also appears generally more to the purpose to employ larger and more powerful instruments for giving, and smaller instruments of more delicate and lighter construction for receiving, at the same time bringing the instrument into the most suitable position for the ear.

Too powerful receiving-apparatus have the drawback, that the countercurrents produced by the vibrations of their membrane weaken the moving currents and displace the trains of sinusoid waves of the induced currents, by which the speech is made indistinct and assumes strange shades of sound.

It is scarcely to be assumed generally that telephones on Bell's principle (in which the sound-waves themselves have to perform the work of exciting the currents required for their conveyance) will be successfully produced so as to utter speech distinctly intelligible at a greater distance from the telephone; and, as we have already insisted, it is quite impossible of attainment that they should reproduce not weakened the mass of sound by which their membrane is struck, or even reinforced. This possibility, however, is not excluded when a galvanic battery is used for putting in motion the membrane of the receiving-apparatus, which then accomplishes the work to be expended. Reis endeavoured to effect this by means of contacts, Edison with the aid of powdered graphite inserted in the conduction-circuit of the battery.

Contacts will hardly operate with sufficient constancy and certainty for the sounds of speech to be given back with purity. But it is possible that the solution of the problem lies in the course taken by Edison; it therein only depends on the discovery of a material or an arrangement by means of which changes in the resistance of the circuit may be produced considerable in amount and proportional to the amplitude of the vibrations of the membrane. The form and quality of graphite powder are too variable to accomplish this with certainty. Experiments which I have commenced with other arrangements have not at present given any satisfactory result. Nevertheless Edison's procedure remains well worthy of consideration, as it possibly forms the key to a future important development of telephony.

If, however, telephonic instruments are susceptible of further extensive improvement, the conducting-lines will always confine the circle of their application within rather narrow limits. Even if, as we have already shown to be indispensable, special posts be appropriated to telephone-lines, carrying no telegraph-wires, and double lines be everywhere employed for the telephones, yet even the telephonic messages on several

lines attached to the same posts would soon, with increasing length of the lines, be disturbed one by another, not only through imperfect insulation permitting side currents to pass over to neighbouring wires, but also through the production of secondary currents in them by electrodynamic and electrostatic induction, generating confusing sounds. In telegraph-lines electrodynamic induction can, as a rule, be entirely neglected, because it does not increase with the length of the line, if the resistance of the wire coils be left out of consideration, and because the duration of the electrodynamically induced currents is too short to affect the telegraphic instruments; but in telephonic apparatus the brief currents generated by voltaic induction produce very audible sounds if the conducting-lines run side by side for only a short distance.

Further, secondary electrostatic induction, increasing as the squares of the length of the conducting-line, will soon, as the overland lines become longer, put a limit to the employment of the telephone, even when the telephone-wires only are fixed to the same posts.

The circumstances are much more favourable in this respect for the telephone when underground or submarine lines are employed. Before I had ascertained that the intensity of the currents which yet are capable of exciting the telephone to the production of clearly intelligible speech-sounds is so extremely slight, I doubted the practicability of employing subterranean wires for great distances, on account of the great weakening which the current-waves called forth by rapidly alternating electromotive forces in the conducting-wires would undergo with the length of the conduction. The experiments, however, which Postmaster-General Dr. Stephan (to whom the German Empire owes the reintroduction of the underground wires that had for a quarter of a century almost fallen into oblivion) caused to be made with Bell telephones, gave the surprising result that with them people can speak with perfect distinctness and quite intelligibly at distances of about sixty kilometres. Hence it is very probable that, with telephones of more powerful action, adequate intelligibility will be attained at twice or even three times that distance. This, at all events, may be the extreme distance at which telephonic correspondence is generally practicable.

Unfortunately, even in underground conducting wires disturbances by return currents from the earth, as well as by electrodynamic and electrostatic induction, are not excluded. The former could be pretty completely got rid of, as in lines above ground, by the employment of entirely metallic conduction-circuits, with the exclusion of the earth as return conductor.

The same holds good in the case of disturbances produced by induction, if the two insulated conductors forming a telephone-circuit are united into a separate cable encased with iron wires. If, on the contrary, as is usually done for the sake of saving expense, a greater number of insulated conductors are combined in one cable, voltaic as well as static induction makes its appearance in augmented measure on account of the slightness of the distance between them, and act very disturbingly on the telephonic correspondence. This secondary electrostatic induction occurs perturbingly even in long cables for telegraphic correspondence, with which very sensitive apparatus must be employed. Hence I have proposed, for avoiding it, to provide the individual conductors which are combined in a cable containing several wires with a conducting metallic sheath in conducting connexion with the outer iron spinning or with the earth. Even encasing the insulated individual conductors with a thin layer of tin foil gets rid of secondary electrostatic induction completely. Any one can easily convince himself of this by experiment if he places one upon the other two mica or thin gutta-percha plates, each lined on both sides with tin foil. If the inner linings be insulated and the charge between the outer ones be tested by the deflection of a galvanometer by connecting the free pole of a battery led away to earth with one of the outside sheets of tin foil, while the second is connected through the galvanometer-wire with the earth, or in a similar manner by aid of the commutator, as great a charge is obtained as if the sheets in the middle were absent. But if the latter are connected with the earth, no trace is obtained of a secondary charge in the tin foil connected with the galvanometer.

We get the same negative result when the individual insulated conductors of a cable consisting of several such have been tightly wrapped round with tin foil or strips of thin plate of any metal. The metallic conductive casing, though very thin, completely prevents any secondary electrostatic induction or charge of one conductor by the charge of another. On the other hand, however, the electrodynamic induction exerted by the wires upon one another is not thereby removed, as Foucault asserted *.

This can easily be convincingly shown by a simple experiment. If two wires, insulated with gutta percha or caoutchouc, be wound together upon a roller, powerful charge as well

* Foucault, on the 2nd of July, 1869, took out a patent in England for encasing the individual conductors with tin foil or other conducting substances, with the expressed purpose of compensating electrodynamic induction by the countercurrents arising in the tin casing.

as voltaic-induction currents are to be observed in one of the wires when a galvanic series is alternately closed and opened by the other. If the roller be now placed in a vessel, and this be little by little filled with water, the charge-currents in the former wire diminish, and cease altogether when the water quite fills up the intervals between the wires, whereas the electrodynamically induced currents become even more intense.

For telegraph-conductions these electrodynamically induced currents are, as we have already remarked, of no consequence, since they do not increase with the length of the conduction; but the telephone, being so extremely sensitive, is still excited by them if the inducing currents are not extraordinarily feeble. It will therefore be necessary to lay down special cables for telephones, just as special posts are needed for them when the wires are carried above ground.

As follows from the above, the telephone is still capable of essential improvement; in a short time telephones will assuredly be constructed which will convey both speech and musical tones beyond comparison more loudly, more distinctly, and with greater purity to moderate distances than they have been hitherto by the Bell telephone. The telephone will then render service to intercourse in cities and between neighbouring towns which will far surpass what the telegraph can perform for short distances. The telephone is an electrical speaking-tube which, just like an ordinary speaking-tube, can be managed by every one, and can be a perfect substitute for personal conversation; but as at very short distances it will never supplant the speaking-tube, just as little will it be able to take the place of the telegraph for greater distances. Yet in the limited circle of its practicability it will soon be numbered among the most important pillars of modern civilization, if external hindrances do not prevent its development and application.

XIV. *On Salt Solutions and Attached Water.*

By Frederick Guthrie.

[Continued from p. 44.]

On the Separation of Water from Crystalline Solids, in Currents of Dry Air.

§ 184. The high water-worth of many of the cryohydrates (§ 88), and the want of evidence of simple arithmetical relationship between the atomic numbers of the water and salt of almost all these bodies, invited me to reexamine a few of the most definite and stable crystalline salts containing water. And this invitation was the more pressing because, in the

matter of the determination of water of crystallization, analysts have for the most part allowed themselves a far greater latitude in respect to agreement between the experiment made and the conclusion drawn than they have been willing to admit in regard to the other constituents of the salt. Prominent in respect to agreement between experiment and the derived constitution are the investigations of Graham, especially on the water of crystallization of single and double sulphates. Such accord is so rare, that in a very great many instances the experiments actually point to a different water-worth than that adopted by the experimenter.

That heat is sometimes liberated and sometimes absorbed when a salt is brought into contact with water, not only according to the nature of the anhydrous constituents, but also to the degree of hydration, has long since shown that there is some essential difference in the tension of the union which is established between the water on the one hand, and the more or less hydrated anhydride on the other. But the statements as to the conditions under which a salt becomes anhydrous, or exists in combination with a definite relative number of molecules of water, are neither definite nor satisfactory. The statement that a salt gives up n molecules of water when heated to the temperature E is inexact, (1) unless the hygrometric state of the air is given, (2) unless it is known whether free circulation takes place, and (3) unless the pressure on the salt is known. The statement that a salt gives up m molecules of water *in vacuo* (over a desiccator) is also ambiguous, in so far as it ignores the temperature.

The salt which I first examined in this respect was chloride of barium, $BaCl_2 + 2H_2O$ ($BaCl + 2HO$), it being a salt easily got quite pure and of a stable nature. The "pure" salt of commmerce was recrystallized, boiled with carbonate of barium, filtered and precipitated, and washed with alcohol. It was then twice recrystallized. It was then finely powdered, and a part A was dried for forty-eight hours in a good vacuum at a temperature of about 17° C.; a part B was dried between repeatedly renewed bibulous paper in a screw-press for the same time. Two analyses of each were made, the elements being estimated in the usual way.

Estimation of Barium.

	Weight of substance.	Sulphate.	Barium.	Per cent. of barium.
A (1) . .	2·7768	2·6912	1·57061	56·563
A (2) . .	1·3997	1·3406	0·78825	56·560
B (1) . .	1·3942	1·3445	0·79054	56·703
B (2) . .	0·8847	0·8480	0·49857	56·360

Estimation of Chlorine.

	Weight of substance.	Chloride of silver.	Chlorine.	Per cent. of chlorine.
A (1) . .	0·6579	0·7640	0·18900	28·72
A (2) . .	0·8559	0·9970	0·24665	28·70
B (1) . .	0·8849	1·0344	0·25590	28·92
B (2) . .	1·3965	1·6344	0·40433	28·91

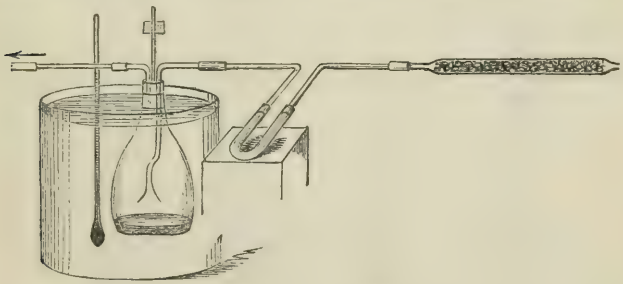
(Ba=137, Ag=108, S=32, O=16, Cl=35·5).

Hence

	Calculated.	Dried *in vacuo*. Mean.	Dried between paper. Mean.
Ba . .	56·139	56·561	56·531
Cl_2 . .	29·098	28·912	28·710
$2H_2O$ . .	14·754	14·527	14·795
	100·000	100·000	100·000

From these analyses, and from the direction of their divergence from the theoretical composition of the salt, there can be no doubt about the composition of the salt ; nor is there any doubt that the water is present in simple molecular ratio. Accordingly the hydrated chloride of barium is admirably adapted for examination as to the conditions under which it gives up water. The only statement I can find in this respect is that the hydrated salt gives up the whole of its water at 100° C.

§ 185. A two-ounce flask with the lip cut off was provided with a glass cap for use alone in the balance-case : the figure explains the rest. A given volume of air (measured by the



quantity of water leaving the gasometer) is drawn in a given time over a thin layer of the hydrated salt, while the latter is heated to a given temperature. The air passes first through a long tube containing fragments of hydrate of potassium, and then through a tube containing glass and sulphuric acid.

TABLE XLI.

Substance = 4·1605.

Volume of air drawn through flask at uniform rate of 3300 cubic centims. in 60 minutes.	Temperature.	Loss.	Loss by 3300 cubic centims.
cub. cent.	°		
26400	17	0·0008	0·0001
3300	25	0·0000	0·0000
,,	40	0·0063	0·0063
,,	,,	0·0073	0·0073
,,	,,	0·0078	0·0078
,,	,,	0·0075	0·0075
,,	,,	0·0075	0·0075
,,	,,	0·0071	0·0071
,,	,,	0·0052	0·0052 (in 45′)
,,	,,	0·0078	0·0078
,,	,,	0·0066	0·0066
,,	,,	0·0062	0·0062
,,	,,	0·0044	0·0044 (in 30′)
,,	,,	0·0060	0·0060
19800	,,	0·0412	0·0069
,,	,,	0·0420	0·0070
,,	,,	0·0346	0·0058
,,	,,	0·0407	0·0068
,,	,,	0·0340	0·0057
9900	,,	0·0105	0·0035
,,	,,	0·0044	0·0015
,,	,,	0·0022	0·0007
,,	,,	0·0020	0·0007
19800	,,	0·0029	0·0005
,,	,,	0·0059	0·0009
,,	,,	0·0000	0·0000
,,	60	0·0063	0·0011
9900	70	0·0099	0·0033
19800	70	0·0256	0·0043
9900	70	0·0090	0·0030
9900	50	0·0000	0·0000
9900	50	0·0000	0·0000
9900	60	0·0038	0·0013
19800	70	0·0210	0·0035
9900	55	0·0000	0·0000
9900	58	0·0000	0·0000
9900	59	0·0000	0·0000
9900	60	0·0038	0·0013
19800	70	0·0187	0·0031
26400	70	0·0259	0·0032
39600	70	0·0411	0·0034
9900	80	0·0239	0·0080
9900	90	0·0483	0·0161
9900	90	0·0358	0·0119
9900	80	0·0151	0·0050
9900	80	0·0182	0·0061
9900	60	0·0010	0·0003
9900	90	0·0021	0·0007
6600	100	0·0000	0·0000

As the flask had to be left for half an hour in the balance-case every time before weighing, the above experiments occupied a few weeks. At the end of this time the flask was found to have lost 0·0017 gram.

Examining the final result as a direct determination of the whole of the water, I found that the 4·1605 grams had lost 0·6087 gram, or 14·63 per cent., instead of the theoretical amount 14·75.

The actual weight of water which a given weight of the salt lost at a given temperature in a given time has little interest, because it is conditioned by the attitude of the salt to the air-current in the flask, and it is also governed by the rate. But points of very great interest are nevertheless presented when the above numbers are compared. The first loss, at 17° C., may probably be attributable to the more complete drying of the salt, since no further loss was experienced at 25°. Starting at 40°, a considerable loss was experienced, which continued with very considerable regularity until the residue approached in composition to the one-atom hydrate $BaCl_2 + H_2O$; the loss then suddenly diminished and abruptly stopped. The total loss experienced by the 4·1605 grams of salt when this point was reached is 0·3009—that is, 7·21 per cent. As the percentage of water in $BaCl_2 + H_2O$ is 7·37, there can be no question that there is a difference in the strength of union of the two water molecules to the salt, or, more exactly, that it requires different physical conditions to separate αH_2O from $BaCl_2$ than are sufficient to separate βH_2O from αH_2O, $BaCl_2$.

At what minimum temperature the β molecule begins to be separated is missed in this Table: it lies somewhere between 25° and 40°. But it appears that when the temperature is such that one molecule begins to be stirred, the whole of that molecule is removed if the current of dry air be continued. In the case of the chloride of barium, there is a range of temperature below 60° and reaching down to the above-mentioned minimum, in which the α molecule is fixed while the β molecule is removable.

The minimum temperature required to disconnect the α molecule is well marked. The salt having ceased to lose weight at 40° C., lost weight distinctly at 60°, and still more rapidly at 70°. On reducing the temperature to 50° no loss could be detected; at 60° the same as before, and at 70° the same as before. At 55° there was no loss, nor at 58°, nor at 59°; but at 60° the original loss was reestablished. And until the salt is becoming anhydrous, for each temperature there is a pretty constant loss.

The anhydrous $BaCl_2$ thus obtained, when mixed with water,

may raise the temperature from 19° to 38°. The solution is perfectly limpid and neutral.

§ 186. In order to make a more systematic attack on the β molecule, a fresh quantity was taken of the salt which had been dried *in vacuo* over sulphuric acid, and had then stood in air over sulphuric acid for three weeks.

TABLE XLII.

Substance = 5·6462.

Volume of air drawn through at uniform rate of 3300 cubic centims. in 15′.	Temperature.	Loss.	Loss by 3300 cubic centims.
9900	26°	0·0005	·0002
,,	29	0·0012	·0004
,,	32	0·0025	·0008
,,	34	0·0053	·0018
,,	36	0·0068	·0023
,,	38	0 0089	·0030
,,	40	0·0129	·0043
,,	50	0·0236	·0079
16500	58	0·1450	·0290
9900	53	0·0446	·0149
,,	50	0·0222	·0074
,,	45	0·0209	·0070 (in 30′)
19800	55·5	0·0821	·0137
,,	,,	0·0192	·0032
9900	,,	0 0022	·0007
6600	,,	0·0014	·0007
19800	56	0·0074	·0012
9900	57	0·0029	·0010
,,	57	0·0022	·0007
19800	57	0·0000	·0000

It seems that the almost inappreciable loss at 26° is really continuous with the greater losses at higher temperatures. The loss at 25° having been shown in Table XLI. to be inappreciable, we may consider the loss to begin between 25° and 26°. It is observed that the rate in Table XLII. is four times as great as in Table XLI.; and a consequence of this is that, for given volumes at the same temperature (40°), the losses are absolutely and relatively to the quantity less in Table XLII. than in Table XLI., but not four times as small. Hence, as we might anticipate, at a given temperature and for a given volume more water is withdrawn by a slow current than by a quick one; while in a given time more water is withdrawn by a quick current than by a slow one. With regard to the first of these facts, it need only be remembered that the slow current becomes more saturated than the quick one. Although the

cessation of loss is for both atoms sufficiently well marked and abrupt, the final balance of water is retained in both cases with considerable tenacity.

In the second series of experiments the flask preserved its weight exactly. The total loss on the 5·6462 grams was 0·4119 gram, showing a percentage loss of 7·277.

The whole analysis now stands as follows, allowing for the loss of the glass in the first series, for the salt dried over sulphuric acid:—

		Calculated.
Ba . . .	56·56	56·14
Cl_2 . . .	28·91	29·10
αH_2O . .	7·44	} 14·76
βH_2O . .	7·23 (7·28 and 7·19)	}
	100·14	100·00

On mixing the body $BaCl_2 + H_2O$ with water a rise from 21° to 27° (or 6°) was obtained. The anhydrous salt with water gave from 19° to 38° (or 19°).

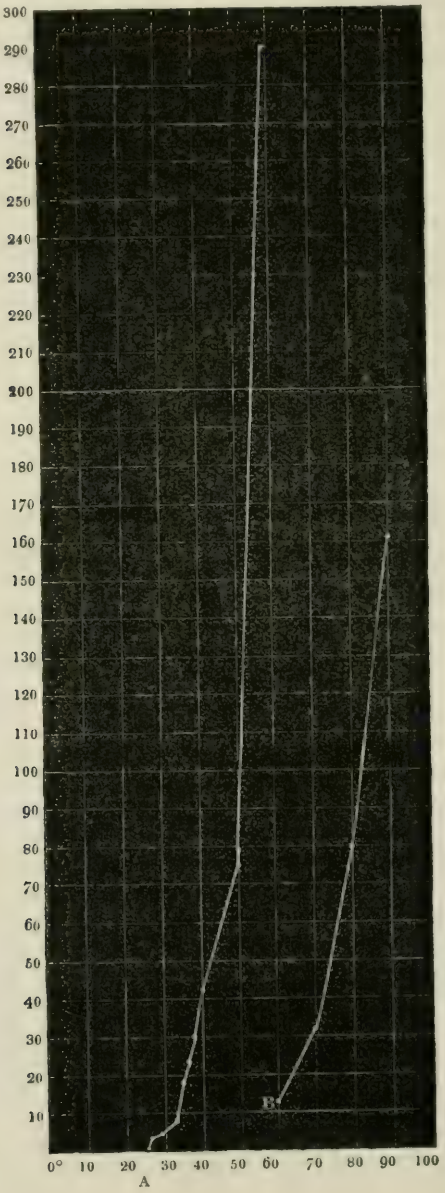
§ 187. Briefly to recapitulate concerning $BaCl_2 + 2H_2O$. At the ordinary barometric pressure, and in a current of air dried and freed from carbonic acid, one water molecule is removed at all temperatures above 25° C., the other at all temperatures above 60° C. In the figure (p. 112) A shows the rates of loss of the β molecule in tenths of milligrams, the ordinates being proportional to such losses. The abscissæ are the temperatures. B shows the losses in like manner of the α molecule.

Chromatic Value of other Media than Water.

§ 188. There are few media besides water which dissolve metallic salts. Amongst the few glycerine stands preeminent; and this liquid is indeed comparable with water itself in its solvent power. On account of this very solvent power, there appears to be at present no evidence of the replacement of water by glycerine in solid hydrated salts, similar to the replacement in siliceous jellies. Some glycerates (using the term homologously with hydrates) are well-defined bodies enough; and the properties of some new ones will be described subsequently. Here I shall confine myself to the description of the effect upon the colours of a few salt solutions, according as the solvent is water or glycerine. Being partially what is called "colour-blind," I have, of course, availed myself of the services of my friends in describing the appearances presented.

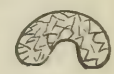
§ 189. Many are entertaining the idea of the relationship between the vibrating periods of the light-wave and the mass of the molecule, simple or compound, of the medium. The

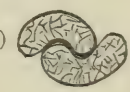
expression "loading" a molecule, by associating it with another more or less massive molecule, may perhaps be the most



convenient term to apply to the cause of the increased period of oscillation which such association entails; but, of course, any systematic obstruction to vibration, any drag, would have the same effect as the drag of inertia.

§ 190. In some experiments relating to the "wandering of the ions" in jellies during electrolysis, which I had the honour of bringing before the Physical Society on the 16th of March, 1878, I described the spreading of the acid and alkaline ions through the unmelted jelly. These results I do not publish, because a friend has had the great kindness to point out to me that many of the results which were then exhibited had been obtained several years previously by Dr. W. M. Ord. The experiments and speculations of Dr. Ord are contained in a very remarkable and suggestive series of papers contributed chiefly to the St. Thomas's Hospital Reports *. Amongst the jellies which I then prepared but did not exhibit, was a stiff gelatine jelly saturated with sulphate of copper. This jelly was of a bright emerald-green by transmitted light. On exposure to the air of a portion which had not been subjected to electrolysis, the water gradually evaporated and the salt began to crystallize out. The form of the crystalline masses was curiously modified by the jelly. Rounded masses were formed, reminding one of my friends of the coccoliths of the deep-sea dredgings—another, of the mineral or uric concretions which occur in mucous media—and yet another, of "chlorite." Whether there be, as is most likely, a common cause, a colloid medium, in all the three cases, I must not here discuss. The ultimate crystalline element is too minute for determination; but the elements of secondary form invariably resemble (1). These elements are frequently linked two by two in one plane (2), or at right angles to one another (3). The convex sides are generally very deeply furrowed, so as to give the impression of their being four second-ray elements. These concretions can be picked out of the jelly in which they form, like almonds out of a cake. So clean is their separation, and so feeble their blackening when heated with oil of vitriol, that they must be regarded as homogeneous bodies free from gelatine; and their composition is therefore

(1) 

(2) 

(3)

* "Some Experiments relating to Forms assumed by Uric Acid" (St. Thomas's Hospital Reports, 1870); "An account of some Experiments relating to the Influence exercised by Colloids upon the Forms of Inorganic Matter" (St. Thomas's Hospital Reports, 1871); "Studies in the Natural History of the Urates" (Rep. Microscopical Society, Jan 6, 1875); "On

of no common interest. Ten of these concretions, which are remarkably uniform in size, weighed 0·5024 gram; the loss on heating to 200° C. in an air-current was 0·1432 gram, showing 28·504 per cent. of water. This points to the formula $CuSO_4 + 3{\cdot}5\ H_2O$; and, as I hope to show in my next communication, the subdivision of the water molecule in hydrated sulphate of copper, or rather the multiplication of the whole formula of that salt, does not admit of doubt. Here again we have evidence of the continuity of composition according to physical circumstances (compare § 142). But what a complete chain of difference of diffusive potential is here indicated, stretching from the first nucleus throughout the jelly! and how it suggests the diffusion and accretion of the matter of a crystalline mineral through a colloid and, perhaps, mechanically rigid mineral matrix!

To return to the green colour of the copper jelly. Gelatine is so complex a body, that although the formation of the blue crystals of the sulphate shows that there has been no general chemical change, yet there is no evidence of its entire absence. Glycerine was therefore next employed as a medium for the solution of various coloured salts.

§ 191. Anhydrous sulphate of copper dissolves so abundantly in glycerine that the solution may be almost solid when cold. There is no sign of crystallization; but the solution at all strengths is a bright emerald-green.

§ 192. Crystals of permanganate of potassium, when heated with glycerine, oxidize it with the escape of gas; but if cold glycerine is added to a cold saturated aqueous solution of the permanganate, a liquid is obtained, without evidence of chemical change, which has been pronounced to be brownish yellow, amber, or, perhaps more accurately, "raw-sienna." In view of the possible chemical change which may be incipient here, it is perhaps better to put this result on one side.

§ 193. Chloride of cobalt, which in water gives the well-known pink hues according to its strength, gives with glycerine a beautiful carmine; this, when heated, is greatly enriched in its blue. When cooled in a carbonic acid cryogen, it acquires a yellowish tint.

§ 194. Chromium potash alum, which in water gives the pale indigo of dilute ink, gives with glycerine an emerald-green.

Whatever be the degree of intimacy of association between the glycerine and the salt, it appears, then, that this association

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some Points in the Natural History of Uric Acid and Urates" (St. Thomas's Hospital Reports, 1875); "Urinary Crystals and Calculi," &c. (Medico-Chirurgical Transactions, vol. lviii. March 9, 1875).

does in all cases retard the light-wave period, or increases its length. The bluing of the cobalt-glycerine solution by heat, and its yellowing by cold also, are entirely in accord with the before-mentioned conception. It may be noticed also that the glycerine solution of a coloured salt is, as a rule, of a much richer colour than the aqueous solution of the same strength, by weight or volume.

I cannot but think that these results lend considerable support to the idea mentioned in § 189, which idea has been so far fruitful in Abney's hands that he has been enabled, by associating a metallic salt with a heavy molecule, to fit the vibrating period of a photographically sensitive film to the light which it is desired to record.

I have to express my indebtedness to Mr. A. K. Huntington for the patience, zeal, and skill which he has shown in helping me in the work of the first half of this part (No. VI.) of my research.

XV. *On the Transmission of Vocal and other Sounds by Wires.* By W. J. Millar, *C.E., Sec. Inst. Engineers and Shipbuilders in Scotland**.

1. *Object of Paper.*—The object of the present paper is the description of a series of experiments made by the author upon the transmission of vocal and other sounds by wires, and the results obtained from those experiments.

2. *Transmission of Sound in general.*—The transmission of sound by various media is familiarly illustrated from day to day; and the readiness with which these media are affected has been made the subject of many experiments.

One familiar illustration of the transmission of sound from air to solids and thence back to the air is that which occurs in the vertical and horizontal partitions between rooms, such as partition walls and floor and ceiling spaces—the sounds originating in one room being thus transmitted to the adjoining room without having recourse directly to air communication.

From a consideration of the latter, as also from other phenomena, the author has for some time been convinced that vocal sounds might be transmitted by solid bodies, such as wires, and that to considerable distances.

After several unsuccessful atttempts, the author during the month of January last, having occasion to use some fine copper wire, carried a portion of it out from the house to a distance of about 20 yards, and attached a couple of pasteboard

* Communicated by the Physical Society.

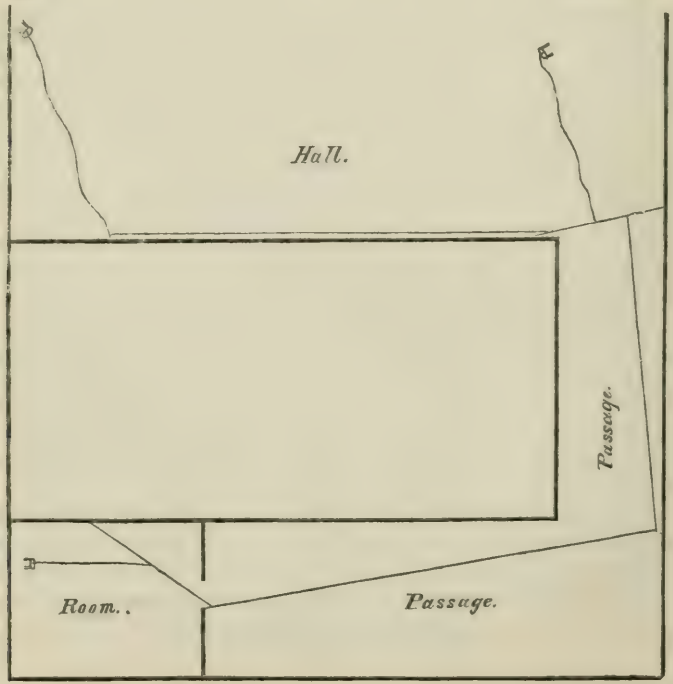
disks with low rims to the ends of the wire: the transmission of vocal sounds was then found to be easily effected, conversation being readily carried on through this length of wire.

Since that time the author has made many experiments with various combinations and under various circumstances. The principle upon which they all more or less appear to depend, so far as the rendering audible of the sounds, is that of the tuning-fork and sounding-box, in which the sound from the vibratory movements of a metal body is considerably intensified when the body is placed upon a sonorous substance affecting the air in its vicinity.

3. *Notes of some of the more important Experiments.*

(1) No. 23 copper wire was stretched between windows outside of house, and attachments at right angles made to rooms through the windows. Speaking in one room was then heard in the other; the distance was about 20 yards. Pianoforte music was easily transmitted by placing an ear-piece inside the instrument and carrying the other end of the wire outside the house.

(2) No. 40 copper wire fitted up in a building, passing from room to room as per diagram below. Six attachments and angles.



Distance about 50 yards. Conversation, singing, whistling, breathing, and the sound of a light C tuning-fork ($2\frac{1}{4}$ inches in fork) readily transmitted.

Various similar arrangements were also made in house from room to room, and finally carried to a distance outside, when all the above effects, as also the transmission of whispering, were clearly demonstrated, the persons at either end being quite out of hearing in the ordinary manner.

The communication was not limited to the persons at either end of the wire; additional connexions were occasionally made, when three or more individuals could communicate with each other.

(3) Carried about 7 yards of No. 23 copper wire from one room through an adjoining one to a room beyond, the wire in its course passing *below two doors* shut above it, and for the most part in *contact with the carpet*, but fastened at the ends so as to produce some tension. Made two connexions of No. 40 copper wire at angles with the main wire; conversation was then readily carried on, and all the phenomena already described produced. Subsequent experiments with No. 16 copper wire arranged as above were found to yield better results.

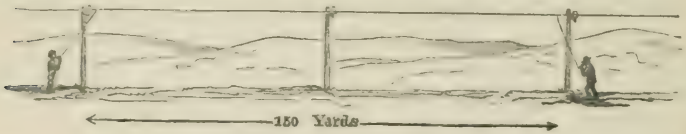
A somewhat similar and equally successful experiment was made by carrying the same size of wire down stairs, passing below two doors and partly resting on carpet and wood. A positive advantage is gained by resting the heavy wire in this manner, the words being clearer and more distinct, and free from the rumbling sound occurring with a suspended wire free to move about.

(4) Fastened No. 23 copper wire to telegraph-wire, made another and similar attachment 75 yards further on, but within two posts. Breathing, whistling, and tuning-fork sounds readily transmitted.

(5) Carried the latter attachment to 150 yards, thus *passing one post.* Breathing, whistling, singing, and the sound of the light C tuning-fork, formerly mentioned, readily transmitted. No apparent loss though passing the support (the latter was of the usual china-ware cup with binding-wire). The speaking was not so distinct, although the different word-sounds were discernible. This can be accounted for by the fact that, as the poles were about 14 feet high, the attachment-ends were free to swing about, which, combined with the exposed situation of the main line, gave rise to a considerable vibratory action due to other causes than the vocal sounds. (See diagram, page 118.)

(6) About 50 yards of No. 23 copper wire was laid out so

as to rest partly on grass, and fastened up at the ends to pins; attachments were made, and vocal sounds transmitted: whis-



tling and the tuning-fork sounds very clearly heard, although a high wind was blowing at the time.

4. *The Mouth- and Ear-pieces.*—The mouth- and ear-pieces used in these experiments have been of various materials and forms. The materials tried have been pasteboard, wood, gutta-percha, india-rubber, parchment, iron, tin, and zinc. These have generally been arranged as disks or drums, having a more or less extended rim around them to confine the sounds. This rim has been of cylindrical, conical, and other forms.

In general, greater volume of sound accompanied increased depth of rim; but the sounds were hardly so distinct as when the rim was kept shallower.

The wire was usually attached to centre of disk; but in some cases good results were got where the wire was led through a cylindrical hollow piece of wood and terminated *close* to the disk; indeed a hollow piece of wood *without* a disk did very well.

As a rule, the effects seemed better when the wire was led outside of the house.

High-pitched voices are more easily heard than deep strong voices.

In the experiments with the telegraph-wire one of the disks used was of thin sheet-iron $3\frac{1}{2}$ inches in diameter. Set in a wooden rim about $\frac{1}{2}$ inch deep, the wire was fastened into a small piece of wood, which in turn was cemented down to centre of disk. The tuning-fork sounds were very well heard with this arrangement; and one peculiarity was that, on the wooden fastening accidentally breaking away from the iron, the sounds could again be heard by holding the disk in one hand and pressing the wooden termination of the wire upon the disk with the other.

5. *Wires.*—The wires, as a rule, require to be more or less tightened up; but this varies with the heaviness of the wire.

The sound is increased with a tight wire.

The volume of sound appears to be increased with a heavy wire. Thus in the telegraph-wire about $\frac{1}{8}$ inch thick, probably No. 8, the sounds were stronger and fuller than in the

thinner wires, and, probably owing to the high tension of the former, faint sounds were more readily transmitted: thus the accidental or intentional touching of the tuning-fork with the rim of the mouth-piece, causing a slight clicking sound, was distinctly heard through the ear-piece at a distance of 150 yards—and this, even although the two attachments of copper wire were practically at right angles to the main wire, whereby part of the sound would pass away onwards up and down the line.

6. The great delicacy of the action may be inferred from the fact that fine sand strewn upon the disk of the ear-piece is unaffected by conversation through lengths of about 7 yards. The sensitiveness also of the mouth-piece was shown by sounds *not spoken into it* being readily transmitted, such as coughing, laughing, or remarks made by persons standing beside the instrument. Indeed, in some cases an advantage is obtained by keeping back from the mouth- or ear-pieces; and the author has sometimes thought an improvement was obtained by holding the ear-piece slightly inclined to the ear.

In all cases the individual voice could easily be distinguished though modified more or less by the structure and material of the mouth- and ear-pieces.

The mouth- and ear-pieces were usually of the same form and material, and were therefore used for either speaking or hearing. Some forms, however, do better as ear-pieces, others as mouth-pieces.

In conclusion, the author believes that many interesting physical questions may be studied by means of these arrangements, and that practical application may be made where communication of this nature is required.

XVI. *On Brass Wind Instruments as Resonators.*

By D. J. Blaikley*.

[Plate I.]

IN bringing before the Physical Society a few notes and experiments on this subject, I would desire to say that they are the result of an attempt to carry somewhat more into detail than, as far as I am aware, has hitherto been done, some acoustical investigations of the late Sir C. Wheatstone. A most interesting paper on Wheatstone's work in this field was brought before the Musical Association by Professor W. G.

* Communicated by the Physical Society, having been read May 25, 1878.

Adams in 1876; and to that paper I am in great measure indebted.

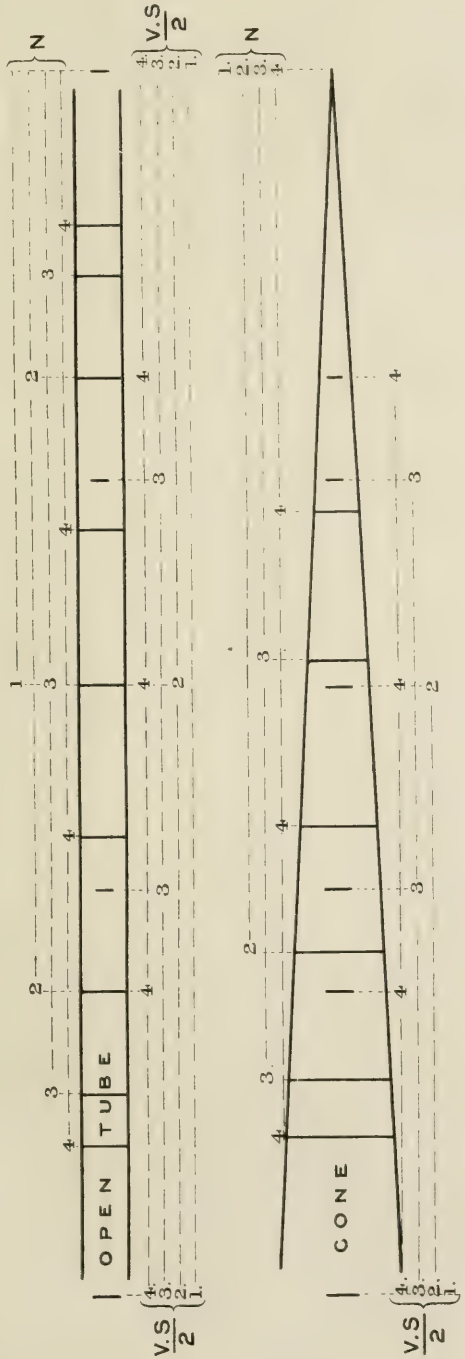
A brass instrument may be defined as a resonator capable of reinforcing a certain fundamental periodic vibration originated by the lips, and all such vibrations as have for their relative numbers 2, 3, 4, &c. when the fundamental note is represented by unity,—these vibrational numbers being the basis of what is known as the natural harmonic series of musical intervals—and this series being the same, whatever may be the absolute pitch of the fundamental note or the character of tone of the instrument.

It is possible to make the lips give notes which, although scarcely audible, are of definite pitch, without the use of an instrument, just as a tuning-fork gives its proper note with or without a resonator.

There are two simple forms of resonators which give the series of notes required in wind instruments: these are the open tube of equal section throughout, and the cone complete to its apex, where it is of course closed. In the tube the wave-length of any note is inversely proportional to its vibrational number; and the nodes or points of maximum compression and rarefaction, and the centres of the ventral segments, or points of maximum amplitude of vibration are equidistant: but in the cone this is not the case. Wheatstone found experimentally that the notes of a closed cone agree in pitch with those of an open tube of the same length; and therefore the prime or fundamental tone of such a cone is an octave higher than the prime of a closed tube of the same length. He found also that in conic frustra of similar lengths, but of different proportions as regards the diameters of their ends, the pitch varied, rising as the difference between the two ends increased when the small end was closed, and becoming lower under the same conditions when the large end was closed. The accompanying diagram (Plate I.) shows the positions of the nodes and centres of ventral segments in an open tube and a cone of the same length for the notes c, c', g', c'', marked 1, 2, 3, 4 (c having 128 vib., and a wave-length of 105 in. at 60° F.). The numerals grouped together and marked N show the positions of the nodal points or surfaces, and those marked $\frac{V.S}{2}$ the centres of the ventral segments or points of maximum vibration. The effect that the diminishing size of the cone has upon the position of the nodes may be easily traced. Whilst the positions of the centres of the ventral segments remain the same as in the open tube (the numerals for these on the cone in the diagram falling exactly under those for the open tube), the nodes

NODAL POINTS.

Scale – $\frac{1}{8}$ full size.



are gradually further and further apart, dividing their respective ventral segments more and more unequally, until at the apex of the cone is a node common to all the notes. It follows from this that the centre of a ventral segment in a cone is not the centre of the length between its nodes, and, conversely, that as the diameters of the two ends of the ventral segment approach equality, so does the position of the node become more central, until the condition of vibration existing in an open cylindrical tube is reached; and such a tube may evidently be considered as a portion of a cone whose apex is at an infinite distance. It is to be noticed that in the cone the number of $\frac{1}{4}$ wave-lengths, or semi- ventral segments, is not directly proportional to the vibrational number as in the open tube, but, with the exception of the fundamental note, is always in excess. Thus let

N = number of $\frac{1}{4}$ wave-lengths,

n = relative vibrational number;

then

$$N = n+(n-1)=2n-1.$$

Instances.—Note 1 (fundamental) $N=1+(1-1)=1$,

Note 4 (double octave) $N=4+(4-1)=7$.

The velocity of the portion of wave or waves in the cone therefore differs with the pitch of the note, and is in no case the same as the velocity in free space. Assuming this latter to be 1120 feet per second, we should have in the cone the following velocities:—

Note.	n.	N.	Feet per second.
c 128	1	1	2240
c' 256	2	3	1493·4
g' 384	3	5	1344
c'' 512	4	7	1280

and the space traversed by the waves of the different notes in one second, measuring from the apex of the cone to, say, the ear of an observer:—

c 128	1122·1875	feet.
c' 256	1121·0937	„
g' 384	1120·7292	„
c'' 512	1120·5468	„

The method I used to find the positions of the nodal points in the cone, and which is applicable to wind instruments or tubes of any varying section, may be illustrated by a conic frustum open at both ends. Holding a vibrating fork over one end (in this case c 512), gradually sink the tube in water:

the water-level when the tube is giving its maximum resonance shows the position of the node.

Brass instruments are generally considered to be cones, or cones combined with cylindrical tubing, neither of which descriptions properly applies; and this I will endeavour to make clear by experiment. We may, in the first place, consider whether the resonance of cones and tubing is influenced appreciably by the action of the lips; and it will be found that, whether the lips or a tuning-fork be used to excite the vibration, the pitch is the same. Two illustrations may be given—the first a common hunting-horn, pitch *c* 512 when it is blown, and giving an excellent resonance to the *c* 512 fork when the mouthpiece is closed; if, however, we slightly alter its length either way, the resonance to the fork is no longer at its maximum. For the second illustration, I take a cylindrical tube which becomes closed on being placed against the lips: blowing it as a wind instrument, we find its proper tones are *c* 128, *g'* 384, *e''* 640, *b''*♭ 896, &c., the same as it would give as a resonator, and that the pitch of these tones is so definite that it is very difficult to alter any of them by the lips more than two or three vibrations, except the lowest. We may here note that the power of a resonator to reinforce the different notes of a series of tones, with the prime or fundamental one of which it is not truly in unison, is much greater for the fundamental than for the higher notes; and this gives the reason for the ease with which the fundamental note of a wind instrument may be varied within pretty wide limits, say half a tone sharper or flatter than its proper pitch. Taking, for illustration, a closed tube 21 inches long, it will be found to give appreciable resonance to a fork of 128 vibrations with quarter wave-length of $26\frac{1}{4}$ inches, but scarcely any to a fork of 384 vibrations (*g'* the twelfth from *c* 128) with quarter wave-length of $8\frac{3}{4}$ inches: when the *c* fork is sounding, the length of the resonance-chamber is to the quarter wave-length as 21 to $26\frac{1}{4}$; but when the *g* fork is used, the corresponding proportion is practically as $3\frac{1}{2}$ to $8\frac{3}{4}$; for in this case there is a second node at a half wave-length, or $17\frac{1}{2}$ inches from the closed end.

For musical purposes a cylindrical tube blown by the lips is evidently unsuited, by reason of its poor tone, as well as by its giving only the odd intervals. The cone gives the required intervals; but it cannot be used by the lips in its complete form; it would be necessary to cut off a considerable portion to get sufficient width for the action of the lips. Assuming the cone shown on diagram to be cut at the second node of note 4 (counting the node at the apex as the first) and there closed by the lips, that note of the original cone can still be

sounded, but no other; the other notes that can be produced may be regarded as the notes 3 and 2 made flatter by their nodes being drawn back, as it were, to the position of node 4, where the cone is cut and the lips are placed; the original notes 2, 3, 4, or c', g', c'', becoming thus the 1st, 2nd, and 3rd notes of a new inharmonic series, with pitches approximately $c\sharp$, $e\flat'$, c''—thus approaching the notes of a cylindrical stopped tube. I have here two other tubes tapering in different degrees—the first two proper tones on the one being c' and $c\sharp''$, and on the other c' and e'' (a major tenth). From these experiments it may be seen that, by using portions of cones of different proportions with their small ends closed, it is possible to get different series of intervals varying between those of an open and those of a closed cylindrical tube—that is, the first interval varying between an octave and a twelfth.

One of the examples just shown (the tube with notes c' and $c\sharp''$) appears to give intervals not very far removed from those required: it may be made use of to illustrate the effect of the combination of a cone with cylindrical tubing, such tubing being of necessity used in practice in connexion with valves or slides to complete the scale. Flattening this cone a fourth, from c' to g, by adding tube, it gives the intervals g, e', d'' in place of the g, g', d'' required, or the ratios 1, $1\frac{2}{3}$, 3 in place of 1, 2, 3, the second interval being actually greater than the first.

These illustrations prove that neither a conic frustum, nor a conic frustum combined with cylindrical tubing, can truly be resonators to notes in the natural harmonic series; but seeing that a bugle or other wind instrument, although it has a considerable diameter at the mouthpiece, may nevertheless be in tune, it appears that its nodal points cannot be in the same positions as those in the cone. On the diagram is represented a bugle of the same pitch as the open tube and cone, with the positions of its nodes and semi- ventral segments as determined by experiment with tuning-forks. Comparing on the diagram the positions of the nodes of any given note in both the bugle and the cone, it will be noticed that there are great differences. The nodes of note 2 show this clearly. Compare lengths from both ends: from mouthpiece to node the length is more nearly equal to that between similar nodes on cylindrical tubing than to that between similar nodes on the cone; but from node to open end it is greater than on the cone, the bugle opening more rapidly.

Thus, then, by altering the proportions of the different semi-ventral segments of which such an instrument may be conceived to be built up, the positions of the nodes may be so

arranged that there shall be a node for every note of the harmonic series at the mouthpiece as required; and according as that is more or less perfectly effected will the instrument be more or less perfectly in tune. This bugle is divided into its seven semi-ventral segments for its 4th note, *c* 512, according to the diagram; and it will be found that by blowing at any one of the nodal points, with any length of the bugle containing an odd number of semi-ventral segments, the note *c''* can be produced. The total number of pieces and combinations that can give this note is eighteen.

Having given these few illustrations of the conditions upon which correct intonation, or the relative pitch of the different notes that can be sounded on a brass instrument, depends, I will now endeavour to show the connexion there is between this point and the question of quality of tone, understanding by quality of tone that characteristic of sound which enables us to recognize a difference between tones of the same pitch. Helmholtz has fully demonstrated that it is only in exceptional cases that we hear a simple musical tone—the vast majority of musical tones being in reality compound tones, in which the fundamental or prime tone has blended with it many upper partial tones of the natural harmonic series,—and that the variety of quality of tone depends mainly upon the number and intensity of these upper partial tones. Blowing the note *c* 256 on three resonators of different forms we get three distinctly different qualities of tone: the resonators now used are a common paraffin-lamp chimney, the conic frustum already shown (having for its first two proper tones *c'* and *c''*), and the bugle. Analyzing these three tones by tuning-forks or resonators, we find that the lamp-chimney of irregular form gives no upper partials; the tone is pure or simple. The cone has the second and third partials sounding, but not strongly, as it is not strictly in tune for them, or, in other words, there is a difference of phase between the prime tone and the partials. And the bugle has all the partials up to the seventh, gradually diminishing in power, but all tolerably strong up to the fifth inclusive. Slightly altering the form of the cone by adding tubing to the narrow end, and maintaining the original pitch of the prime tone (*c* 256), by cutting a portion off the wide end the pitch of the second tone may be altered until it is *c* 512, an exact octave from the prime; and we find that the quality of tone of the prime or fundamental note is altered, owing to the more perfect resonance which the cone now gives to its second partial. In its original form, with proper tones *c'*–*c''*, the cone could give but an imperfect resonance to *c''*, the second partial to its prime *c'*.

In the trombone and the euphonion we have two instruments of very different and characteristic qualities of tones—the trombone being brilliant and piercing, and the euphonion mellow. We may take the B♭ of about 120 vibrations (army pitch) on each of these and endeavour to give a visible proof of the existence of high upper partial tones. For this purpose I use small tubular resonators covered at one end with a tightly strained diaphragm or tympanum of goldbeater's skin, against the centre of which is hung a very small bead, or drop of sealing-wax, by means of a single thread of cocoon-silk. The two resonators now used are tuned respectively to the fourth and ninth partials of B♭, or *b'*♭ of 480 vibrations and *c'''* of 1024. When the proper tone of such a resonator is sounded in its neighbourhood, either as a simple tone or as a partial in a compound tone, the agitation of the membrane puts the bead in violent motion, which can easily be seen in the image thrown on the screen by the electric lamp. It will be noticed that when B♭ is sounded, either on the euphonion or on the trombone, both resonators are agitated—but that the excursions of the bead due to the partial tone of 1024 vibrations are much greater with the latter than with the former instrument, although both are played with but moderate force, thus proving that, though partials as high as the ninth exist in the quality of tone produced by both these instruments, yet in the trombone the upper partials have much greater strength than in the euphonion. With a resonator more suitable for private experiment than these, I have distinctly heard the sixteenth partial tone in the B♭ of the trombone. A tapering tube open at both ends, or a common wine-bottle with the bottom knocked out, is very convenient for analyzing tones. Sinking such a tube in water and holding the ear close against the small end, the various partials existing in a given compound tone may be readily discerned, as the length of the tube changes according to the depth it is immersed in the water.

Although one wind instrument may be made to approach another in quality of tone by means of different methods of blowing, and it is therefore not so easy to analyze the tones of these as it is those of keyed instruments with fixed tones, yet I have endeavoured to establish some general data ; and these I will lay before you. The instruments the tones of which I have analyzed are the B♭ tenor trombone, the B♭ euphonion, the F French horn, the B♭ cornet, and the bugle. The partial tones named in the Table are those heard when the instruments are gently blown ; with loud blowing higher tones can be discerned. The ordinary marks of musical expression, *pp*, *p*, *mf*, *f*, are added in cases where I found it possible to make a comparison.

Instrument.	Note.	No. of vibrations.	Partial tones heard.
B♭ Trombone ...	B♭	60	1, 2, 3, 4, 5, 6, &c. to 16. *p*
	B♭	120	1, 2, 3, 4, 5, 6, &c. to 12. *p*
	f	180	1, 2, 3, 4, 5, 6, 7. *p* [than 12 weak.
B♭ Euphonion ...	B♭	60	1, 2, 3, 4, 5, 6, &c. to 12; higher *p*
	B♭	120	1, 2, 3, 4, 5, 6, 7. *f mf p*
	f	180	1, 2, 3, 4, 5. *p*
F French horn ...	*c*	135	1, 2, 3, 4, 5, 6, 7, 8. *mf p pp*
	f	180	1, 2, 3, 4, 5, 6. *mf p*
	f	360	1, 2, 3, 4, 5, 6. *p p*
B♭ Cornet	*bb*	240	1, 2, 3, 4, 5, 6, 7, 8. *f f p p*
Bugle	...	256	1, 2, 3, 4, 5, 6, 7. *f mf p pp*

While submitting that the different qualities of tone are accounted for by the difference in the number and force of the upper partials in any given compound tone, I must at the same time acknowledge that I can do no more than throw out a few suggestions with respect to the causes that influence the production of such upper partials in this remarkable manner. The partials being in the natural harmonic series, it is evident that if the various proper tones of a vibrating column of air such as is enclosed in a wind instrument are not in exact agreement with this series, the resonance to the partials cannot be at its best. Take for illustration two instruments nominally the same (say two bugles), but with somewhat different qualities of tone. Suppose that a certain compound tone on both should have its first and second partials of equal intensity, but that one instrument has that one of its proper tones that is nearest in pitch to the required second partial a semitone sharper than that partial; the supposed compound tone sounded on that instrument will be deficient in the quality the second partial should give.

As regards instruments of different characters, the chief points influencing the tone are the general form of the instrument (understanding by this the proportions of the column of air, and not the shape into which the instrument may be bent up for the convenience of the player), the extent of the flanging of the bell, and the form of the mouthpiece. As an

illustration of the first of these conditions the trombone may be compared with the euphonion; the tubing of the trombone is cylindrical for about two thirds of its length from the mouthpiece, but the euphonion opens with gradually increasing curvature from the mouthpiece to the rim of the bell. The high upper partials being more powerful on the former than on the latter instrument, it would appear that the cylindrical tubing has the power of maintaining the intensity of the short waves to a greater extent than the tapering tubing has. The bell-flange may be increased in size to a considerable degree without altering the pitch of an instrument; but such increase has a marked effect on the quality of tone, greatly subduing the force of the upper partials. I find by experiment that the pitch is not altered by the extension of the flange curvature beyond a point at which its tangent would make an angle of about 40° with the axis of the instrument, although the quality of tone is decidedly altered by such extension. This may be illustrated by changing the bell-end of a bugle for a bell with much wider flange, more like that of a French horn: comparing the two, it will be noticed that the change in quality of tone is very marked.

The form of the cup of the mouthpiece varies for different instruments, from that of a long deep conical funnel to that of a comparatively shallow well-rounded cup—the first form representing the French-horn mouthpiece, and the second the mouthpiece for instruments of brilliant tone, as the trumpet and trombone; those for cornets, bugles, and saxhorns are of an intermediate character. Although it is manifest that a shallow cupped mouthpiece favours the production of high upper partials, I have not as yet succeeded in arranging any experiments which would illustrate the cause of this. One fact, however, noticed by many observers, appears to me to be suggestive, and worth bearing in mind in connexion with this subject. It is this:—If a vibrating tuning-fork be placed on a sounding-board, the quality of tone it gives varies with the pressure applied: touching the board very lightly with the fork the prime tone is well heard; but on pressing the fork down to the board the tone appears to jump up an octave; at least the second partial (octave of the prime) is heard with great distinctness. This experiment appears to prove that if an elastic resonant body (in this case the resonant board) is in a state of initial pressure at the point of origin of vibrations, a vibration that would otherwise be simply pendular becomes a vibration compounded of two or more simple pendular vibrations. Applying this consideration to wind instruments, and bearing in mind the initial pressure caused by the escape

of air from the lips, it would appear probable that mouth-pieces of different forms so modify this initial pressure as to cause a variety in the number and intensity of the upper partial tones.

XVII. *On the Nebular Hypothesis.*—IX. *Radiation and Rotation. By* PLINY EARLE CHASE, *LL.D., S.P.A.S., Professor of Philosophy in Haverford College*.*

[Continued from vol. v. p. 367.]

AMONG the most interesting of the unsolved astronomical problems are the questions as to the origin of solar radiation and of cosmical rotation. These two problems, as I have already shown, are intimately connected, at the centre of our system, by the ultimate equality which exists between the velocity of light, the limiting centrifugal velocity of solar rotation, and the velocity of complete solar dissociation.

It has been commonly assumed that physical forces tend to ultimate equilibrium and consequent complete stagnation. The imperfections of any plan which looks to such a final result have led some writers to suppose that there may be some compensating provisions, hitherto undiscovered, for a renewal of activity. In the search for such provisions, the equality of action and reaction, and the possibility that the compensation is continually furnished by Him who is ever "upholding all things by the word of his power," seem to have been wholly overlooked.

If we assume the existence of a luminiferous æther, whether as a reality, or as a convenient representative of coordinated central forces, its undulations, when obstructed by inert centres, would necessarily lead to such phenomena as those of gravitation, light, heat, electricity, magnetism, &c. Confining ourselves for the present to the action of gravitation, it is well known that the limiting velocity of possible gravitating action and consequent centrifugal reaction at any given point is $\sqrt{2gr}$, the velocity varying as $\sqrt{\frac{1}{r}}$. If, according to the hypothesis of Mossotti, each particle is provided with a definite æthereal atmosphere, the density of that atmosphere in a condensing nucleus should vary as $\frac{1}{r^3}$. But, according to Graham's law,

* Communicated by the Author, having been read before the American Philosophical Society, June 21, 1878.

$v \propto \sqrt{\frac{E}{D}}$. Therefore, in order to satisfy the conditions of gravity, the æthereal elasticity, within any nucleus which is either wholly or almost wholly gaseous, $\propto \frac{1}{r^4}$.

Since such is the supposed character of the solar nucleus, it seems not unlikely that, the centrifugal radiations of any heavenly body being at all times equivalent to the centripetal radiations which it intercepts, solar and stellar light and heat are only the reactionary consequences of such perpetual internal oscillations as the æther has first transmitted to the luminous orbs and then resumed. The fact that the reaction which is shown in the centrifugal force of solar rotation, and the action which is shown in parabolic orbital velocities, find a common limit in the velocity of light, may perhaps be regarded as a crucial test of this hypothesis, which is further strengthened by the following considerations.

In the huge comet-like nebulosity which is indicated by the solar-stellar paraboloid, the interesting relation which has been pointed out by Stockwell* between the perihelia of Jupiter and Uranus, and the many indications of normal "subsidence" which I have shown in previous papers, suggest the probability of an early ellipsoidal nucleus with subordinate nucleoli—the major axis of the nucleus being bounded by $2\Psi_5$ (60·939) and $2\mathfrak{H}_5$ (41·358), and the Sun being in the focus. The *vis viva* of condensation would give velocities of incipient orbital separation at Ψ_5 (30·470) and $\mathfrak{H}_5$ (20·679); and $\mathfrak{U}_1$ would then be in the centre of the entire system ($30{\cdot}470 - 20{\cdot}679 \div 2 = 4{\cdot}885$; $\mathfrak{U}_1 = 4{\cdot}886$), even as $\oplus_3$ is nearly in the centre of the secondary system ($\overline{\sigma_5 + \varphi_1} \div 2 = 1{\cdot}017$).

If we apply Gummere's criterion ($n = 11{\cdot}656854$), we find that three prominent centres of "subsidence" were determined by this early ellipsoidal nucleus. For $2\Psi_5 \div n = 5{\cdot}228$, $\mathfrak{U}_3$ being 5·203; $2\mathfrak{H}_5 \div n = 3{\cdot}548$, which is near the outer limit of the asteroidal belt, $(107)_3$ being 3·560; $(\Psi_1 - \mathfrak{H}_1) \div n = 1{\cdot}022$, the centre of the secondary system being, as above stated, 1·017. The Earth is still in the centre of a "subsidence" ellipsoid, of which the Sun is in one focus, while the outer asteroidal region (3·2028) and $\mathfrak{U}_3$ (5·2028) are at opposite apsidal extremities of the major axis. Moreover 3·2035 is the extremity of an atmospherical radius which would move with the velocity of light, provided the sun's surface were moving with orbital velocity, or the velocity of incipient dissociation ($\sqrt{gr}$).

* Smithsonian Contributions, xlv. p. 232.

It seems probable that, in consequence of subsidence, Jupiter, which, as we have already seen, was the centre of nucleal volume, may have also been the centre of nucleal mass at the time of its complete orbital separation, and that it was therefore the primitive Sun of the extra-asteroidal planets before it became our Sun's "companion star." For with the present mass of the system, and with a mean radius vector $= \Psi_1 + \mathcal{U}_1$ (34·4845), the orbital period of Neptune would be 73,966 days. Two successive subsidences ($34{\cdot}4845 \div n^2$) would bring the solar nucleal surface to about $\frac{2}{3}$ of $\mathcal{E}_3$, or 54·53 solar radii. The angular acceleration of rotation, due to subsequent nucleal contraction, would $\propto \frac{1}{\gamma^2}$. Therefore, when the Sun had contracted to its present limits, its rotation-period would be $73966 \div 54{\cdot}53^2 = 24{\cdot}88$ days*.

If this were the only coincidence of its kind, we might perhaps have some good grounds for looking upon it as merely curious and accidental; but the bond of connexion which we have already found between rotation and revolution, in the limiting formative undulations which are propagated with the velocity of light, may prepare us for accepting evidences of a similar bond in the phenomena of nebular subsidence.

There are three other known systems of cosmical rotation which may help us to judge as to the rightfulness of such an acceptance, viz.:—that of the extra-asteroidal planets, with an estimated average period of about 10 hours; that of the intra-asteroidal planets, with an estimated period of about 24 hours; and that of the moon, with a synodic period of 29·5306 days. If these periods are dependent upon the same subsidence which led to the early belt-formations, we may reasonably look for evidence of that dependence of a character similar to that which we have found in the case of the sun.

We have seen that the first subsidences from 2Ψ and $3\mathcal{U}$ account for the orbital ruptures of Jupiter and Earth; secondary subsidences from points within the orbital belts account

* These relations may have an important bearing on Croll's hypothesis of the origin of solar radiation. In the stellar-solar paraboloid, of which traces still exist between Sun and α Centauri, there must have been frequent collisions. Some of Croll's critics have shown strange misapprehensions as to the possible velocity of collision. The limit of possible relative velocity, from the simple gravitation of two equal meeting masses, is $2\sqrt{2gr}$. This would be equivalent, taking the values of g and r at Sun's apparent surface, to ·01774 r, or more than 750 miles per second. If projection were added to gravitation, or if the two masses had small solid nuclei of great density, while the greater part of their volume was gaseous, or if there were a large number of equal masses, the limit of possible velocity might be largely increased.

for these three rotation-periods. For $\mathcal{U}_5 \div n = 101{\cdot}73$ solar radii, and Jupiter's orbital revolution $(4332{\cdot}585 \text{ d.}) \div 101{\cdot}73^2 = 10{\cdot}05$ h.; $\oplus_4 \div n = 19{\cdot}66$ solar radii, and Earth's orbital revolution $(366{\cdot}256 \text{ d.}) \div 19{\cdot}66^2 = 24{\cdot}205$ h.; $\mathfrak{D}_4 \div n = 5{\cdot}442$ Earth's radii, and Earth's rotation $\times 5{\cdot}442^2 = 29{\cdot}619$ d. In these accordances we have additional evidence of the equality of action and reaction.

The normal character of rotation is still further traceable, even after the formation of the subordinate planets in the two principal planetary belts. If we seek the point of incipient condensation which would lead to such rotation-periods as have been generally assigned by astronomers to the different planets, we readily find that Gummere's criterions, Newton's third law, and the law of equal areas lead to the formula $n\left(\frac{T}{t}\right)^{\frac{1}{2}} = \frac{R}{\rho}$, in which n = Gummere's criterion, $\frac{T}{t}$ = number of planetary rotations in one orbital revolution, R = radius of nebular contraction, ρ = Sun's present radius. Taking Herschel's values for T and t, we have

$n\left(\frac{T}{t}\right)^{\frac{1}{2}}$.	$\frac{R}{\rho}$(1).	$\frac{R}{\rho}$(2).
☿ 110·4	$☿_5$ 102·4	$\frac{1}{2}\oplus_4$ 111·1
♀ 177·6	$♀_5$ 166·4	$\frac{1}{2}♂_4$ 176·6
⊕ 223·1	222·2	
♂ 301·8	$♂_2^1$ 301·5	
(3) 445·4	(3) 427·1 to 719·4	$2\oplus_4$ 444·4
$\mathcal{U}$ 1192·5	$\mathcal{U}_5$ 1185·9	
♄ 1829·5	$♄_1$ 1876·7	
♆ 3245·7		$\frac{1}{2}\Psi_4$ 3258·9

It thus appears that—

1. All the points of incipient condensation, $\frac{R}{\rho}$(1), are within Kirkwood's "spheres of attraction."

2. In the pair of extra-asteroidal planets which are nearest the asteroidal belt, the incipient points are near the *secular* aphelion of the inner, and the *secular* perihelion of the outer planet.

3. In the pair of intra-asteroidal planets which are nearest the asteroidal belt, the incipient points are near the *mean* aphelion of the inner and the *mean* perihelion of the outer planet.

4. The sum of the radii of nebular contraction for the two principal planets of the solar system $(1192{\cdot}5 + 1829{\cdot}5 = 3022)$ is almost precisely equivalent to the sum of the mean perihelion radii of the same planets $(\mathcal{U}_2\, 1069{\cdot}6 + ♄_2\, 1950{\cdot}4 = 3020)$.

5. The secondary points of incipient condensation, $\frac{R}{\rho}(2)$, are all referable, through the simple accumulation of *vis viva*, to primary mean aphelia.

6. The significance of the fourth accordance is increased by Stockwell's discovery* that "the mean motion of Jupiter's node on the invariable plane is exactly equal to that of Saturn, and the mean longitudes of these nodes differ by exactly 180°."

7. Gummere's criterion confirms the theory of Democritus, that the evolution of worlds was due to a vortical movement, which was generated by the descent of the heavier atoms through the lighter.

XVIII. *The Electrical Properties of Bees'-wax and Lead Chloride. By* W. E. Ayrton, *Professor in the Imperial College of Engineering, Tokio, Japan*†.

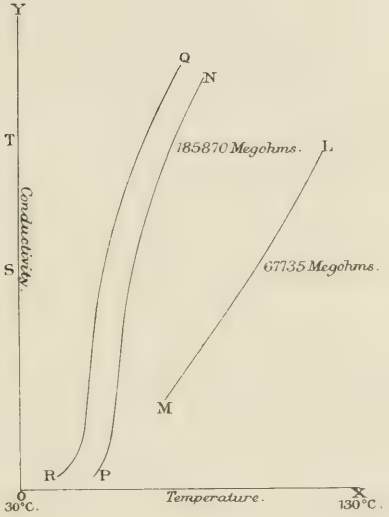
[Plate II.]

In the two papers by Prof. Perry and myself on Ice as an Electrolyte, recently read before the Physical Society, we showed that both the conductivity and specific inductive capacity of $\left\{\begin{matrix}\text{ice}\\\text{water}\end{matrix}\right\}$ increases regularly, without discontinuity, in passing from several degrees below the freezing point to several degrees above it. We drew attention to the fact that, in consequence of the absorbed charge in water being immeasurably greater than the surface-charge, we could not hope, by any method of experimenting, to properly compare the true specific inductive capacity with the index of refraction for light of infinitely long waves; so that, in fact, the only support that Prof. C. Maxwell's electromagnetic theory of light could hope to derive from these experiments must be based simply on the fact that in $\left\{\begin{matrix}\text{ice}\\\text{water}\end{matrix}\right\}$ both the specific inductive capacity and the index of refraction increase as the temperature rises. At the meeting of the Society on November 3rd, at which the second of our two papers was read, Prof. G. Foster mentioned that he had recently been collecting all the results he could find connecting specific inductive capacity with index of refraction. I therefore beg to forward, as a contribution to this collection, the following results of some further experiments which I have been making on this subject.

* *Loc. cit.*

† Communicated by the Physical Society.

Fig. 3.



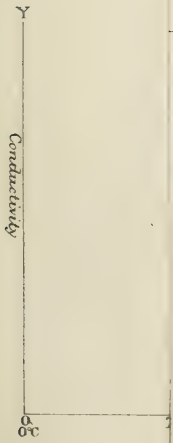
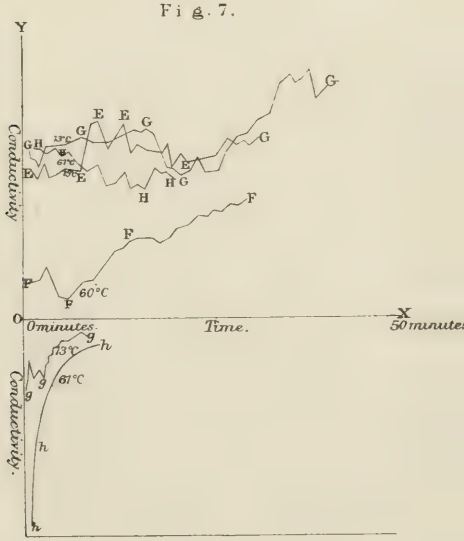


Fig. 7.



Mintern Bros lith.

For the last two years my attention has been turned to wax as a good material for electrically testing, especially in regard to the connexion which, in our paper on the Viscosity of Dielectrics, communicated to the Royal Society, we pointed out existed between high specific inductive capacity and low specific resistance. I therefore had constructed a large condenser, consisting of many sheets of letter-paper soaked in melted bees'-wax, with alternate sheets of tin-foil. After the condenser was built up in the usual way, melted bees'-wax was poured in, the plates squeezed together, and the whole shut up in a fairly good water-tight wooden box. The condenser was buried several feet under ground, to ensure uniformity of temperature, connexion being made with the insulated coating by a piece of Atlantic-cable core, and with the other coating by a piece of bare copper wire.

After this condenser had been buried for a short time underground, it showed the apparently abnormal condition of diminution of resistance by electrification. This phenomenon then formed the subject for special investigation with this condenser, an account of the results obtained being given at the end of this short paper.

As bees'-wax is one of the few substances in which the index of refraction for light increases in passing from the liquid to the solid state, it seemed important, in connexion with the electromagnetic theory of light, to carefully measure the specific inductive capacity of a wax condenser as it was gradually cooled through the solidifying point. A small, shallow, clean copper box, 19 centims. long by 17 centims. wide, was therefore lined with a sheet of letter-paper, 0·036 centim. thick, previously well soaked in melted bees'-wax. A clean copper disk, 12·8 centims. in diameter, was placed on the top, weighted down, and the dish filled up with melted wax. This condenser, A B (fig. 1), was heated in an oil-bath, C D. E and F are holes for the insertion of thermometers, of which the bulbs were just above the wax condenser; G is an opening through which the insulated electrode H of the condenser may protrude without touching the oil-bath. A wooden stand, W W, supports the condenser in the middle of the bath; and a glass vessel V holds strong sulphuric acid to keep the inside space artificially dry. The bath is closed by a double door, which is made to fit well by a strip of leather inserted between it and the bath.

The condenser having been inserted, the bath was heated to about 90° C., and kept at that temperature for some time; the lamps were then removed, when the temperature fell very

slowly. The capacity was then measured (several observations being made at each temperature) by charging the condenser with 75 Daniell's cells joined in series, and discharging it through an exceedingly delicate Thomson's reflecting galvanometer. The curves A B C D, E F G, H I K (fig. 2) represent the results obtained on three different days, distances measured parallel to O X representing temperature, the points O and X corresponding respectively with 0° C. and 100° C., and distances measured parallel to O Y representing capacity, the zero-line for capacity for the curves A B C D, E F G, H I K being below O X by a distance equal to $\frac{2}{3}$ of O Y. For the curve G H I the zero-line is O X. It will be seen at once that these curves, obtained on different days, do not give the same capacity for the same temperature (the numbers, therefore, that have been calculated for the specific inductive capacity are not given); but considering, first, the very small capacities that had to be measured, and, secondly, the difficulty of accurately determining the temperature of a non-heat-conductor like wax, even when enclosed in the oil-bath, the discrepancies in the curves are not to be wondered at. One fact, however, is very striking in all the three curves; and that is the *rise* in capacity as the temperature very slowly falls from about 80° to 60° C., and the subsequent diminution in the capacity on a still further diminution of the temperature. Probably, had experiments on capacity been made when cooling from a much higher temperature, there would have been observed, first, a gradual diminution in capacity due to cooling down to about 80° C. (traces of this first diminution are seen in the portion A B of the curve A B C D); then we have the rise of capacity as the wax solidifies at about 60° C.; and, lastly, we see the subsequent rapid decrease on further cooling. Now this is precisely in agreement with the changes known to occur in the index of refraction for light; and hence the interest of these experiments.

As there was always a small electromotive force in the wax condenser, and as the vibrations of the galvanometer-needle were, as usual, damped by the air-vane, I used for calculating the capacity the formula developed by Prof. Perry and myself for employment in such cases*, which is

$$Q=\frac{H}{lG}\frac{P}{2\sqrt{\pi^2+L^2}}\left[\left\{(x_1-x_r)^2\epsilon^{\frac{2L}{\pi}\tan^{-1}\frac{\pi}{L}}-\frac{\pi^2}{\pi^2+L^2}x_r^2\right\}^{\frac{1}{2}}+\frac{L}{\sqrt{\pi^2+L^2}}x_r\right],$$

* 'A Test for determining the Position of a partial Discontinuity, without Earth-fault,' by Professors W. E. Ayrton and John Perry, p. 12.

where Q is the quantity of electricity discharged through the galvanometer, H the horizontal intensity of the galvanometer-field, l the half length of the needle, G the galvanometer-constant, P the periodic time of vibration, L the logarithmic decrement, x_1 the first sudden swing on discharge, x_r the deflection that would be produced by the small constant current, and which is equal to

$$\frac{x_1 + Dx_2}{1+D},$$

where x_2 is the second swing and D the decrement.

It might, of course, be at once objected that the rise in capacity as the wax cools from about 80° to 60° C. is perhaps not due to any change in the specific inductive capacity, but merely indicates that the distance between the copper plate of the condenser was slightly diminished by the wax shrinking on solidifying. This solution, however, is improbable, since, although a sudden expansion of the wax on solidifying (if such an expansion existed) might have separated the plates, it is unlikely that the contraction which really occurred could have brought them nearer together than the thickness of the paper by which they were separated when the wax was liquid. Nevertheless, partly to obtain additional evidence on this point, and partly to measure the specific resistance (or resistance per cubic centimetre) of bees'-wax at different temperatures, I made ten distinct sets of experiments, occupying many days, on the conductivity of wax. In these experiments the wax condenser was heated up to about 130° C., and a current sent through it with the 75 Daniell's cells in series. The temperature was kept constant at about 130° C. until the galvanometer-deflection had reached its maximum, when it was considered that the wax had acquired the temperature indicated by the thermometer. The temperature was then allowed to fall very slowly, and frequent readings of the galvanometer and thermometer were taken. The results obtained are shown on fig. 3, temperature being measured parallel to O X, the points O and X corresponding with 30° and 130° C.; distances measured parallel to O Y represent conductivity on such a scale that for the curve L M, representing the conductivity between 115° and 65° C., O S corresponds with a resistance of 67,735 megohms per cubic centimetre; and for the curves N P, Q R (which are drawn on a larger scale) the distance O T represents a resistance of 186,000 megohms,—the zero-line for conductivity for all three curves being O X. The point P, corresponding to a resistance of 37,000,000 megohms per cubic centimetre,

represents the lowest conductivity I was able to measure with certainty directly with the galvanometer. It may here be mentioned that such high resistances could be measured with the galvanometer, since one Daniell's cell, through a resistance of 600 megohms, gave 130 scale-divisions deflection on a scale about $1\frac{1}{2}$ metre distant. The various curves obtained for conductivity between 115° and 65° C. agree so closely that they may all be represented by the one curve L M. The curves for the conductivity between 80° and 40° C. are all quite regular, but not all of exactly the same slope, the difference depending on the highest temperature to which the wax was heated before cooling on the particular day of experimenting, this being sometimes about 130° C. and at other times about 90° C. All the curves, however, agree so closely that they are all contained between the two limiting curves N P and Q R shown in fig. 3. In no curve was there *the slightest appearance* of a rise of conductivity at the melting-point, which would probably have been obtained had the copper plates approached one another an appreciable distance on the wax solidifying.

We may therefore conclude that in the previous experiments the rise in the capacity at melting indicates a true increase in the specific inductive capacity coincident with an increase in the index of refraction for light.

As regards apparent increase of resistance by electrification, which, as mentioned, I observed during repeated experiments, extending over some months, with the wax condenser buried underground, the general conclusions arrived at were that not only did the conductivity usually increase by electrification, but that it steadily increased day by day—a result indicating that the wax was deteriorating, probably from damp penetrating through the joints of the wooden box in spite of leather having been inserted between the different parts of the wood before they were screwed together. This conclusion appeared to be correct, since, on digging up the condenser and keeping it near a fire for many days, it regained its original high resistance. The damp must therefore not only have entered through the joints in the wood, but through some small cracks that were observed in the mass of wax when the condenser was opened ; and it was probably due to this damp that the peculiar effects of polarization were observed similar to those noticed by the Comte du Moncel when testing stones, and by Mr. T. Warren in certain insulating oils.

I now made a number of experiments with lead chloride as a dielectric—a substance to which my attention was especially drawn by some remarks of M. Buff in *Ann. Chem. Pharm.* cx. p. 258 (1859), in which he says that this substance conducts

electricity like a metal (that is, without decomposition)—a conclusion, however, which at first sight would appear to be negatived by certain experiments of M. Wiedemann published in *Pogg. Ann.* cliv. 318–320, from which he found that the resistance of lead chloride diminished by increase of temperature.

I first had made a small carbon box containing a carbon plate, but prevented from touching it by three small pieces of clean glass. The carbon plate had a carbon electrode attached to it, the whole being cut out of a solid piece of carbon so as to have the shape of an inverted T. Into the box lead chloride was poured in a fused state until it covered up the plate, but leaving the carbon electrode of the plate protruding for connexion with the battery. The whole was then allowed to cool very slowly. The outer part of the carbon box and the end of the carbon electrode were now electrotyped, and copper wires soldered on, the junctions of the carbon and copper being quite clear of the lead chloride.

With this condenser the results given on the next page were observed.

In addition to the resistance of the condenser being measured while the battery was connected, time-readings were also taken with the galvanometer of the discharge from the condenser after the removal of the battery, as well as time-readings with an electrometer of the electromotive force in the condenser producing the discharge. As, however, the diminution in the discharge-deflection was in each case quite regular, the curves are not given.

It is interesting to observe in the Table (p. 138) that in *every* case there is a diminution of resistance by electrification, although in some cases there was an increase during the first minute. Looking at the first group of tests, taken between November 13th and 15th, we see a gradual increase in the resistance day by day. Looking also at the second group made between November 21st and December 1st, we likewise see a steady daily increase in the resistance ; but in the interval between November 15th and 21st, when no tests were made, there appears to be a decided diminution in resistance. Considering, however, that the tests taken on November 13th, at the beginning, and on December 1st, at the end of the investigation, give almost identical results, it cannot be concluded with certainty that there was any decided deterioration taking place in the lead chloride. But on breaking up the condenser, the lead chloride was found to contain many small holes ; so that it is possible that damp may have collected in these. This solution, however, would at first sight appear to be rather

No. of exp.	Date.	Temp.	Battery.	Resistance of the wax condenser, in megohms, after 15 sec.	after 30 sec.	after 1 min.	After	Remarks.
1.	Nov. 13th ...	18°·8 C.	31 volts copper pole to carbon box.....	7·97	8·077	7·875	14 min., 6·774	Steady diminution in resistance from the middle of the first minute.
2.	Nov. 13th ...	18°·8 C.		.....	7·848	8·458	14 min., 6·326	Steady diminution in resistance from the end of the third minute.
3.	Nov. 14th ...	19°·9 C.		.....	10·15	9·706	5 min., 8·682	Steady diminution in resistance from the first observation.
4.	Nov. 15th ...	18°·5 C.		9·254	9·118	9·323	8 min., 7·607	Steady diminution in resistance from the end of the first minute.
5.	Nov. 21st ...	.....	30 volts copper pole to carbon box.....	5·477	5·128	5·128	14 min., 3·951	Steady diminution in resistance from the beginning.
6.	Nov. 24th ...	.....	30 volts copper pole to carbon plate ...	4·874	5·770	.....	12 min., 4·365	Steady diminution in resistance from the middle of the second minute.
7.	Nov. 26th ...	.....	30 volts copper pole to carbon box.....	5·755	6·167	6·167	8 min., 5·820	Steady diminution in resistance from the beginning of the second minute.
8.	Nov. 27th ...	.....		5·911	6·186	5·911	9 min., 4·708	Steady diminution in resistance from the middle of the first minute.
9.	Nov. 28th ...	.....		6·174	6·174	6·174	6 min., 5·409	Steady diminution in resistance from the beginning of the second minute.
10.	Nov. 29th ...	.....		.....	.....	6·476	6 min., 5·440	Steady diminution in resistance from the beginning of the second minute.
11.	Nov. 30th ...	.....		7·875	8·100	7·875	10 min., 6·163	Steady diminution in resistance from the middle of the first minute.
12.	Dec. 1st	.....		8·621	8·493	8·129	5 min., 7·295	Steady diminution in resistance from the commencement.

Condenser well discharged between every two tests.

improbable ; since the condenser, both when being tested and when not tested, was kept in an atmosphere kept partially dry with sulphuric acid.

Some fresh lead chloride was prepared in the same way as before—that is, by precipitating from a solution of lead acetate with a solution of common salt, and carefully washing the precipitate several times with distilled water. But the new condenser, made with the carbon box and plate, was found to have a resistance of only two megohms at 16° C.; and, unlike what was experienced in the former case, the resistance in all the experiments was now found to increase by electrifications. This, however, is probably explained by the fact that, whereas in the previous case thirty volts electromotive force was used, now only one volt was employed ; so that decomposition (whether of the lead chloride itself, or of the damp which after decomposition may act on the chloride) was probably not produced. Some preliminary temperature-tests were now made; and, in accordance with Wiedemann's results, I found that the resistance diminished with elevation of temperature.

The condenser was now left in an atmosphere dried with sulphuric acid from December 21st to January 7th, when it was found that the resistance had increased to about 15·6 megohms at 70° C. A large number of measurements of the conductivity at different temperatures, using the same oil-bath for raising the temperature as is shown in fig. 1, were now made. The different experiments gave results so nearly agreeing that they may all be represented by the curve S T V (fig. 4), in which distances parallel to O X represent temperature, the points O and X corresponding to the temperatures 0° and 100° C. respectively ; and distances parallel to O Y represent conductivity on such a scale that the point T, corresponding to a temperature of 70° C., represents a resistance of about 15·6 megohms. The curve is approximately logarithmic; that is, the ratio of the difference of the logarithms of the conductivities to the difference of temperatures is approximately constant.

Thinking that possibly the method previously employed for making the lead chloride, by precipitating it from a solution of lead acetate with a solution of common salt, may have introduced traces of some salt of lead other than the chloride, the following method was now employed for making another supply of the chloride. From a clear solution of lead nitrate a precipitate was formed with colourless hydrochloric acid, and the precipitate well washed with distilled water. A third condenser was now constructed, three small pieces of glass 0·225 centimetre thick being used to separate the carbon

plate from the box. The connexions with the box and plate were made as before, by first electrotyping the carbon with copper and then soldering on copper wires, the junctions of the carbon and copper not being in contact with the lead chloride. A condenser was also made with a copper box and a copper plate, the two being well coated with graphite to protect them from the action of the lead chloride, and separated from one another by three small pieces of glass 0·135 centimetre in thickness.

Tests of conductivity were now made at different temperatures with both condensers, 0·075 volt being employed with the carbon and 7·5 volts with the copper condenser, or sometimes 2·2 volts with the latter. With the carbon condenser and with the smaller electromotive force the resistance was found to increase with electrification, whereas with the copper condenser and with the electromotive force 2·2 volts the resistance diminished with electrification, and with 7·5 volts this diminution became much more rapid, these results being observed at both high and low temperatures. In the earlier experiments with this copper condenser the diminution was regular, whereas later on it proceeded irregularly; but on the whole it may be said that when an electromotive force not exceeding $1\frac{3}{4}$ volt was employed there was an increase in resistance by electrification, such as is usually experienced with gutta-percha and with ordinary dielectrics, while when the electromotive force exceeded this limit there was either a regular or an irregular diminution of resistance by electrification—the results apparently not depending much on whether carbon or copper coated with graphite was used for the plates of the condenser.

As this limiting electromotive force appears to be about the same as that necessary to decompose water, I think we may fairly conclude that the diminution in resistance is due to a decomposition of the damp (which appears to be contained in the lead chloride even when careful means are taken to dry it), and to the products of the decomposition acting on the chloride. Fig. 5 shows four electrification-curves, A A A, B B B, C C C, D D D, obtained from four *successive* experiments with the carbon box, and corresponding with the temperatures 15° C., 57° C., 14° C., 61° C. respectively. Time is measured parallel to O X, the points O and X corresponding to the moment of applying the battery, and to 50 minutes afterwards; conductivity is measured parallel to O Y from the line O X of zero conductivity. The curves B B B and C C C, for 57° C. and 61° C., are drawn on a vertical scale five times smaller than that employed for A A A and C C C—the scale for

time, however, remaining the same. All the tests from which these curves are drawn were made with 0·075 volt electromotive force. The curves *a a a*, *b b b*, *c c c*, fig. 6, are the discharge-curves obtained in the three above experiments for 15° C., 57° C., and 14° C., observations of the discharge in the experiment for 61° C. not having been taken. The scale both for conductivity and for time is the same exactly as that employed in the curves A A A, C C C, fig. 5. All the curves show a regular increase of resistance with electrification, the increase being far more rapid at a high than at a low temperature. As was seen from the curve S T U, fig. 4, so also from fig. 6 we learn that the conductivity is much greater at a high than at a low temperature; and we also see from the curves A A A, B B B, C C C, D D D, that the general effect of testing day by day appears to lower the conductivity.

Electrification-curves E E E, F F F, G G G, H H H, fig. 7, were obtained from four *successive* tests with the copper-box condenser, and correspond with the temperatures 15° C., 60° C., 13° C., 61° C. respectively. Time is measured parallel to O X, the points O and X corresponding to the moment of applying the battery; and 50 minutes afterwards conductivity is measured parallel to O Y from a zero as far below O X as the point Y is above. Curve F F F is on a scale for vertical distances one twentieth of that employed for the curves E E E G G G, and H H H on a scale one fifth of that used with E E E, G G G: that is to say, if the same scale were employed for vertical distances for all four curves, the two for the higher temperatures would be far above those for the lower temperatures—in fact, would be off the paper altogether. For horizontal distances (that is, for time) the same scale is employed for all the curves. An electromotive force of 7·5 volts was employed with all the experiments from which these four curves were drawn. Although the curves are irregular, still, on the whole, there is an increase of conductivity or diminution of resistance with electrification; and that this is probably due to the chemical action of the current referred to above is shown from the irregularity of the discharge-curve *g g g* obtained after removing the battery in the test at 15° C. Curve *h h h*, however, which is the discharge-curve for the test at 61° C., does not show any such irregularity; but then it must be noticed that H H H, the charge-curve for this temperature, indicates on the whole rather an increase than a diminution of resistance by electrification.

April 1st, 1878.

XIX. *Theory of Voltaic Action.* *By* J. Brown, *Esq.**

THE production of a difference of electric potential by voltaic action is attributed by some primarily to the difference of chemical attraction between the two elements of a voltaic couple for one of the components (ions) of some compound body (electrolyte) in contact with both, that element which has the greater affinity being the positive one. It is said by others to be due to the simple "contact" of the two elements without the intervention of any third substance or combination, and has been attributed, in the case of two metals such as copper and zinc, to their mutual chemical attraction†. Faraday could not, however, discover any current during the combination of two metals (tin and platinum), through great heat was evolved‡; and he considered that though the source of energy in a voltaic pair was the combination of the active ion with the positive plate, decomposition was necessary to its development in the form of electricity. Numerous old experiments may be cited which show how in various ways alterations in the electric relations of metals in voltaic pairs may be produced without altering their contact.

The following experiments seem to go far towards establishing the truth of the first-mentioned (chemical) theory.

If a potential series (A) be formed by immersing couples of various metals &c. in an oxidizing electrolyte and testing for the current generated, and another (B) by the use of condenser-plates in the usual way adopted by contact theorists, the two series will be found curiously similar. The simplest conclusion appears to be that the so-called "contact" excitement is due to the presence of a gaseous film § containing water, carbon dioxide, or other oxygen compounds between the plates, which film may be considered as having all the properties of an oxidizing electrolyte except its conductivity.

If in forming series A we use an electrolyte containing some other active ion such as sulphur, we obtain a totally different series, which, as Professor Fleeming Jenkin remarks‖, is "quite anomalous and inconsistent with the simple potential theory." But if the chemical theory be true, then in forming series B, if we substitute for the ordinary atmosphere, containing watery vapour and other oxygen compounds, an

* Communicated by the Author.

† Sir William Thomson, 'Electrostatics and Magnetism,' § 400; Tait, 'Recent Advances,' p. 305 *et seq.*

‡ Phil. Trans. 1834, p. 436.

§ Wiedemann, *Galvanismus*, p. 12.

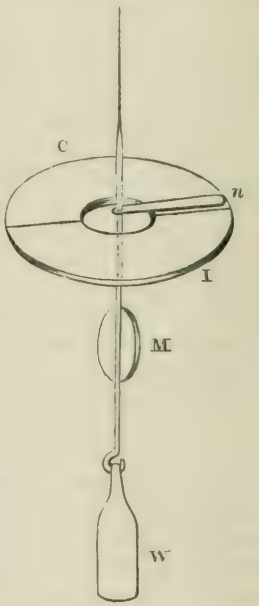
‖ Electricity and Magnetism, p. 217.

atmosphere containing a suitable sulphur compound, the anomaly should disappear, and we should obtain effects with the condenser which would place the metals in the same potential order as when immersed in sulphur electrolytes. In order to verify this the following experiment was made. Starting with the fact that iron is positive to copper in an oxidizing electrolyte (as water), while copper is positive to iron in a solution containing potassium sulphide or other similar sulphur compound, I made a condenser with disks $4\frac{1}{2}$ inches in diameter, one of copper, the other iron, well ground together. The iron disk was screwed on the lower end of an iron rod sliding in a brass tube fixed with shellac in a wooden cover fastened on the neck of a gas-jar. The jar stood on a wooden stand, through the middle of which rose a similar insulated rod carrying the copper disk. Means were provided for adjusting the disks parallel to one another, and also for filling the jar which enclosed both disks with any required gas.

To measure the charge excited by the "contact" of the plates a quadrant-electrometer was employed, which gave a deflection of 5 millims. for the potential of a bichromate cell. When the condenser-plates were placed together (in ordinary atmosphere) connected with opposite pairs of quadrants and then separated, the index light moved over 1 centim., the iron being positive, as was to be expected. Hydrogen sulphide was then allowed to flow into the gas-jar; and on repeating the connexion and separation of the plates the iron proved to be negative, the light-spot moving over about 3 centims. in the direction opposite to its first motion. This was repeated several times; and on examining the plates after the experiment, the copper was found to be of a deep blue colour, while the iron was scarcely altered. It will be observed here that the only alteration in the circumstances of the experiment was the change in the atmosphere surrounding the plates. The contacts all remained the same; and when the atmosphere contained a sulphur compound, the plates assumed the same electric relation as they would in an electrolyte containing a sulphur compound. Even the proportionate degree of tension between the plates in air and in hydrogen sulphide is similar to the ratio of their electromotive force in water and in potassium sulphide solution.

The next experiment appears to confirm in a marked way the view that the difference of potential between two metals in contact is due principally, if not altogether, to the difference of their affinities for one of the elements of some compound gas in the atmosphere surrounding them. The experiment is a modification of one devised by Sir William Thomson, and

described in his 'Papers on Electrostatics and Magnetism,' p. 317:—"A metal bar insulated so as to be movable about an axis perpendicular to the plane of a metal ring made up half of copper and half of zinc, the two halves being soldered together, turns from the zinc towards the copper when vitreously electrified, and from the copper towards the zinc when resinously electrified." Instead of a copper and zinc ring I used a copper and iron one, C I, 3·1 inches diameter outside, with a 1 inch hole in centre. It was supported on a tripod inside a case with plate-glass sides, and which could be connected by rubber tubing with an apparatus for generating hydrogen sulphide. A piece of lead-paper was placed inside the case to detect the first entrance of the gas. From the top of the case rose a vertical glass tube with a torsion head, from which depended a platinum wire ·0025 in. in diameter and about 19 in. long, carrying the needle or bar, *n*, of thin sheet aluminium, $1\frac{1}{2}$ in. long by $\frac{3}{16}$ in. wide, a mirror M of about 4 ft. focus, and a glass weight, W, which dipped in a vessel of water to steady it. The tripod carrying the ring rested on the points of three screws passing up through the bottom of the case, by means of which the plane of the ring could be adjusted so as to get equal deflections on each side of the zero-line. The needle hung at a distance of 1 or 2 millims. above the ring, as nearly as possible over the junction of the metals, and having its suspension-wire in the centre of the ring. It was electrified by connecting it with the positive or negative conductor of a Winter's plate machine.



In a preliminary experiment with a copper-zinc ring, deflections of 5 centims. on each side of zero on the scale were readily obtained. With the copper-iron ring, however, the deflections were only $\frac{1}{2}$ to 1 centim., the iron being as zinc to the copper. As the potential of the needle could not be maintained constant by the means employed, the deflection was continually varying in amount; but when the machine was carefully worked these variations were slight, and did not interfere with the result. What follows is from my notes of the third time of going over the experiment. The needle being

negatively electrified and deflection about ½ centim. towards iron, the case was connected with the hydrogen-sulphide bottle, and sulphuric acid poured in to generate the gas. At 2½ minutes afterwards the lead-paper began to darken at edge; and in half a minute more the needle crossed the zero-line and turned towards the copper half of ring—deflection about ⅓ centim. The needle was then connected with positive conductor and immediately turned towards iron; connected again to negative it turned towards copper; and so on, till in about 10 minutes after admitting the gas the deflections became undecided, the copper having become covered with sulphide, which has no affinity for sulphur.

Edenderry House, Belfast,
January 1878.

XX. *Notices respecting New Books.*

Astronomical Observations made at the Royal Observatory, Edinburgh, Vol. XIV., *for* 1870–1877. *By* Piazzi Smyth, *F.R.S.E. &c. Published by order of Her Majesty's Government.* Edinburgh: Neill and Co., 1877.

In this volume Professor Smyth has given his attention to a most important feature of Sidereal Astronomy, viz. "Stellar Proper Motions." At present we know but little either of the distribution of the stars in space or of the directions in which they are moving. Proctor has shown that groups of stars separated, as seen by the unassisted eye, many degrees from each other, possess a community of motion, the logical inference being that they are in some way connected. The spectroscope reveals to us the fact that many stars are receding from us in the line of sight, and that others are approaching us in the same line. Proctor's deduction of "Star Drift" is based, if we mistake not, on the Proper Motions as given in various catalogues which he found necessary to study in constructing his star-maps; but neither the present recorded Proper Motions nor the recorded motions in the line of sight give us any information as yet as to the distribution of the stars in space; and if Professor Smyth's suggestion be carried out, of filling up the lacunæ purposely left in his catalogue for the reception of observations of R.A. and N.P.D. from other sources to which he has not had access, it is extremely probable that many corrections may be made to the numerical values of Proper Motions now on record. Whether the Astronomer Royal for Scotland has succeeded in truly correcting the values on record or not, he has at all events drawn the attention of astronomers to the subject, and that in a way which cannot fail of contributing to its advancement.

It is considered, by astronomers competent to give expression to

a sound judgment, that the best determination of Proper Motions which we possess are by Mr. Stone; they are contained in vol. xxxiii. of the Memoirs of the Royal Astronomical Society, and are derived from a comparison of Bradley's observations with the Greenwich seven-year Catalogue for 1860, giving an interval of 105 years, on which the determinations rest.

It is greatly to be desired that the Proper Motions of the Stars, as now recorded in various catalogues, should be most scrupulously examined, Stone's determination being taken as the basis. Among the stars we have a remarkable class, viz. those having *large* Proper Motions. Professor Newcomb, in his 'Popular Astronomy,' refers to the star 1830 Groombridge as the most remarkable of them, its Proper Motion being more than seven seconds of arc per annum, which, combined with its parallax, gives a real motion of two hundred miles in a second of time. There are two stars said to have a greater Proper Motion than this star, viz. 2151 Navis, whose Proper Motion is 7″·9 per annum, and ϵ Indi, 7″·7. By giving close and unremitting attention to these stars, light may be thrown on the question as to whether they are members of a group passing through our sidereal system with immense velocity. If Professor Smyth's Catalogue contributes in any degree to elucidate our knowledge of stellar Proper Motions on a great scale in our sidereal system, he has not done his work in vain. We greatly approve of the plan he has adopted of leaving spaces for the insertion of observations from other sources, also of devoting a page to each star; and we shall look with great interest for the appearance of the next four hours of the Catalogue.

XXI. *Proceedings of Learned Societies.*

GEOLOGICAL SOCIETY.

[Continued from p. 73.]

May 8, 1878.—Henry Clifton Sorby, Esq., F.R.S., President, in the Chair.

THE following communications were read:—

1. "On the Glacial Phenomena of the Long Island, or Outer Hebrides." 2nd paper. By James Geikie, Esq., LL.D., F.R.S., F.G.S.

In this paper the author gave some additional notes on the glaciation of Lewis, and a detailed account of the glacial phenomena of Harris and the other islands that form the southern portion of the Outer Hebrides. Additional evidence was adduced to show that Lewis has been glaciated from S.E. to N.W.; and the shelly boulder-clays and interglacial shell-beds of that part of the Long Island were described in detail. Harris, North Uist, Benbecula, South Uist, Barra, and the other islands that go to form the chain of the "Long Island" were successively described under the headings of Physical features, Geological structure, Glaciation, Till or

Boulder-clay, Erratics and perched blocks, Morainic débris and Moraines, Freshwater lakes and Sea-lochs. Numerous bearings of striæ, which abound, were given; and these were held to prove that the whole Outer Hebrides have been glaciated by ice that flowed outwards from the mainland of Scotland. The position of abundant *roches moutonnées* points to the same conclusion; and this is still further supported by the "travel" of the Till. That deposit is generally absent or very sparingly present on the rock-faces that look towards the mainland; but it is heaped up in their rear, and spreads over the lower tracts that slope gently towards the Atlantic. On the west side of the islands not a few boulders occur in the Till which have been derived from the east; and the same is true of certain erratics lying loose at the surface of the ground. The islands are well glaciated up to a height of 1600 feet above the sea; and the line of demarcation between the glaciated and non-glaciated areas is extremely pronounced. Above 1600 feet the hills show rugged, splintered, jagged, and sometimes serrate tops. The author regarded the Till or boulder-clay as the morainic material that gathered underneath the ice; and proof of this is given. Erratics and perched blocks are very numerous; and most of these, as well as much of the morainic *débris*, are believed to have been dropped where we now find them during the final melting of the ice-sheet. It was shown, however, that certain erratics and perched blocks and some well-marked moraines are due to local glaciers, as are also some of the striations in a few of the mountain-valleys. The origin of the rock-basins which are now lakes was discussed, and attributed to the erosive action of ice. To the same cause were assigned the rock-basins which occur in certain of the sea-lochs.

In concluding, the author pointed out that we may now arrive at a true estimate of the thickness attained by the ice-sheet in the north-west of Scotland. If a line be drawn from the upper limits of the glaciations in Rosshire (3000 feet) to a height of 1600 feet in the Long Island, we have an incline of only 1 in 210 for the upper surface of the ice-sheet; and of course we are able to say what thickness the ice reached in the Minch. Between the mainland and the Outer Hebrides it was as much as 3800 feet. No boulders derived from Skye or the mainland occur in the Till of the Outer Hebrides; and this was explained by the deflection of the lower portion of the ice-sheet against the steep wall of rock that faces the Minch. The underpart of the ice that flowed across the Minch would be deflected to right and left against the inner margin of the Long Island; and the deep rock-basins that exist all along that margin are believed to have been scooped out by the grinding action of the deflected ice. Towards the north of Lewis, where the land shelves off gently into the sea, the under strata of the ice-sheet were enabled to creep up and over the district of Ness, and thus gave rise to the lower shelly boulder-clay of that neighbourhood, which contains boulders derived from the mainland. The presence of the overlying interglacial shell-beds proves a subsequent melting of the ice-sheet,

and a depression of the land for at least 200 feet. The overlying shelly boulder-clay shows that the ice-sheet returned and overflowed Lewis, scooping out the older drift-beds and commingling them with its bottom moraine. The absence of kames was commented upon, and shown to be inexplicable on the assumption that such deposits are of marine origin, whilst if they be of torrential origin their absence is only what might be expected from the physical features of the islands. The only traces of Postglacial submergence are met with at merely a few feet above present high-water mark.

2. "Cataclysmic Theories of Geological Climate." By James Croll, Esq., LL.D., F.R.S. Communicated by Prof. Ramsay, LL.D., F.R.S., F.G.S.

The author commenced by calling attention to the great diversity of the hypotheses which have been brought forward for the explanation of those changes in the climate of the same regions of the earth's surface which are revealed by geological investigations—such as alterations of the relative distribution of sea and land, of the ecliptic, and of the position of the earth's axis of rotation—all of which, he maintained, have proved insufficient or untenable. Sir William Thomson has lately maintained that an increase in the amount of heat conveyed by ocean-currents, combined with the effects of clouds, winds, and aqueous vapour, is sufficient to account for the former prevalence of temperate climates in the Arctic regions: and this view, the author stated, he had himself been contending for for more than twelve years. He thinks, however, that alterations in the eccentricity of the earth's orbit is the primary motive cause, whilst Sir William Thomson believes this to be the submergence of circumpolar lands, which, however, in Miocene times, appear to have been more extensive than at present. He pointed out that a preponderance of equatorial land, as assumed by Sir Charles Lyell to account for the milder climate of Arctic regions in Miocene times, would rather tend to loss of heat by rapid radiation into space, whilst water is remarkably powerful as a transporter of heat; so that, in this case, equatorial water rather than equatorial land is needed.

In speaking of the glacial climate, the author maintained that local causes are insufficient to explain so extensive a phenomenon. He indicated that we are only too prone to seek for great or cataclysmic causes: and although this tendency has disappeared from many fields of geological research, this is not the case in all. His explanation of the causes of a mild climate in high northern latitudes is as follows:—Great eccentricity of the earth's orbit, winter in perihelion, the blowing of the south-east trades across the equator perhaps as far as the tropic of Cancer, and impulsion of all the great equatorial currents into northern latitudes; on the other hand, when, with great eccentricity, the winter is in aphelion, the whole condition of things is reversed: the north-east trades blow over into the southern hemisphere, carrying with them the great equatorial currents, and glacial conditions prevail in the northern hemisphere. Thus those warm and cold periods which have prevailed during past geological

ages are regarded by the author as great secular summers and winters.

3. "On the Distribution of Ice during the Glacial Period." By T. F. Jamieson, Esq., F.G.S.

The author believes that a study of the distribution of ice during the Glacial period proves that the greatest accumulations of snow took place in precisely those districts which are now characterized by a very heavy rainfall; and he pointed out how exactly this is in accordance with the views of Prof. Tyndall as to the conditions most favourable to the development of glaciers. In support of this conclusion he reviewed the phenomena presented by the most highly glaciated districts of the British Islands, of Scandinavia, and Europe generally, and of Asia and North America, and contended that in every case his opinion is borne out, the districts which are now remarkable for an excessive rainfall having been formerly centres of dispersion for great systems of glaciers. The notion of a polar ice-cap he held to be opposed to many well-known facts; and he discussed the distribution of various forms of life during and since the Glacial epoch, with the object of determining whether the drainage of ice from the great polar basin was effected by means of the depression of Davis's Straits or of Behring's Straits. The evidence appeared to him to be in favour of the former channel.

May 22.—Henry Clifton Sorby, Esq., F.R.S., President, in the Chair.

The following communications were read:—

1. "On the Serpentine and associated Igneous Rocks of the Ayrshire Coast." By Prof. T. G. Bonney, M.A., F.G.S., Professor of Geology at University College, London, and Fellow of St. John's College, Cambridge.

In a paper published Q. J. G. S. xxii. p. 513, Mr. J. Geikie states that the rocks of this district are of sedimentary origin, a felspar-porphyry being the "*maximum* stage of metamorphosis exhibited by the felspathic rocks," and the diorite, hypersthenite, and serpentine being all the result of metamorphism of bedded rocks. This view is also asserted in the catalogue of the rocks collected by the Geological Survey of Scotland. The author had seen specimens of rocks from this district which so closely resembled some from the Lizard, that he visited the Ayrshire coast in the summer of 1877. The conclusions formed in the field have since been tested by microscopic examination. He finds that several, at least, of the group of "dioritic" rocks are of igneous origin, and are dolerite and basalt, since they contain augite, not hornblende. The serpentine is undoubtedly an intrusive rock, the evidence being abundant and remarkably clear. One specimen can hardly be distinguished at sight from the black serpentine of Cadwith (Lizard); the resemblance also is most striking when the rock is examined chemically and microscopically. Examination of different varieties shows the serpentine to be, like that of Cornwall, an altered olivine-enstatite rock. The rock called

hypersthenite is also intrusive. The author found no hypersthene. There are two varieties:—one a remarkable rock, consisting mainly of large crystals of diallage, a gabbro extremely rich in this mineral and almost free from felspar; and a gabbro of later date, much resembling the ordinary gabbro of the Lizard, the felspar being converted into a kind of saussurite, and some of the diallage into hornblende. The "felspar-porphyries" appeared to the author in the field to present all the characters of true igneous rock, to be associated with tuffs, and to be unconformable with the above-described group of rocks. Microscopic examination placed their igneous character beyond doubt. There are also some basalt dykes of later date than the above. The author is accordingly of opinion that the principal conclusions of the paper referred to above are not warranted by either stratigraphical or lithological evidence. He considers it probable that the "felspar-porphyry," like so much of that in Scotland, is of Old-Red-Sandstone age, and that the serpentine is of later date, but Palæozoic.

2. "On the Metamorphic and overlying Rocks in the neighbourhood of Loch Maree, Ross-shire." By Henry Hicks, M.D., F.G.S.

The rocks in the neighbourhood of Loch Maree have been described by various authors, but chiefly and most recently in papers communicated to the Geological Society by Prof. Nicol, of Aberdeen, and by Sir R. Murchison and Prof. Geikie, of Edinburgh. The views held by these authors in regard to the order of superposition of the rocks are well known to be greatly at variance, not only as regards some of the minor subdivisions, but in relation to the actual age of nearly the whole of the rocks to the east, or those forming the Central Highlands. The older geologists and, more recently, Prof. Nicol hold the view that the Central Highlands consist almost entirely of the old fundamental (pre-Cambrian) gneiss, or rocks of that age; whilst others, represented by the late Sir R. Murchison and by Prof. Geikie, say that the rocks forming the whole of the Central Highlands are of much later date, and for the most part of Silurian age. In the present communication the author endeavours to show, from results obtained by him recently by a careful examination of a section extending from Loch Maree to Ben Fyn, near Auchnasheen, that the interpretations previously given are in some important points incorrect, and that this has been to a great extent the cause of such very diverse opinions.

The section described by him runs for some miles along the north shores of Loch Maree, is then continued in a S.E. direction along the heights opposite Kilrochewe and across Glyn Laggan, and then in an easterly direction through the heights on the north of Glyn Docherty to Ben Fyn and the range of mountains to the north of Auchnasheen.

On the western and for some distance along the north shores of Loch Maree the Lewisian rocks (fundamental-gneiss series) are seen to consist chiefly of reddish or greyish gneiss and hornblende- and mica-schists. The strike in these beds is more or less continuous

from N.W. to S.E., varying occasionally to N. and S.; and they dip generally at a high angle and are much contorted. Resting unconformably upon this gneiss series, and forming here the upper part of the mountain Slioch (about 4000 feet high), are the Cambrian conglomerates and sandstones, made up chiefly of masses of the rocks below cemented together by a comparatively unaltered matrix. In this, however, he found masses of other rocks, very similar to those found in the Cambrians of Wales, and which he thinks must have come from beds of an intermediate age (like the Pebidian series in Wales), which have either been completely denuded off here, or must be present in some other area not far distant. These beds are, for the most part, nearly horizontal; but on the east side they dip slightly to the S.E., where they are succeeded unconformably by the quartzites of Crag Roy. (These quartz rocks are also beautifully exhibited on Ben Eay, to the south of Loch Maree, and resting unconformably on the Cambrian rocks of the magnificent Torridon Mountains.) Alternating with these rocks are some of the so-called fucoidal bands, the beds all dipping with a considerable inclination to the S.E. Upon the quartzites are seen the Limestone bands, occupying chiefly the sloping ground on the west side of Glyn Laggan. These are penetrated by a great mass of granitic rock, which produces here considerable contact-alteration, the limestone, however, at some distance from the mass being in a comparatively unaltered state. In all sections across Glyn Laggan hitherto described the mass of intrusive rock is made to penetrate along the bedding, and is supposed to separate the Limestone entirely from the upper series of rocks, the so-called Upper Gneiss, &c. The author, however, found another series of sandstones, calcareous grits, and blue flags beyond the main intrusive mass, and occupying a considerable portion of the gradually descending ground between the river and the heights on each side. These were also penetrated by another arm of the granite, but, as in the case of the limestone, with the sole result of altering them near the junction. Prof. Nicol places a fault at this point, and says that the fundamental gneiss is here brought up to give an appearance of overlying conformably the unaltered series. The author, however, holds, with Sir R. Murchison and Mr. Geikie, that the next is a younger series, and that it truly overlies the unaltered beds: but he entirely demurs to the view held by them that these should in any way be called gneiss rocks, or associated in any way with beds which have undergone the metamorphic change so characteristic of the pre-Cambrian rocks as known in this country, and which could only be induced, he believes, by great depression combined with heat, moisture, and pressure. On examination he found these upper beds everywhere unaltered, except near dykes; and the change there induced in them was that now well known as contact-alteration, and which is so entirely distinct from true metamorphism. These beds all dip to the S.E., and attain a thickness of several thousand feet. They are flag-like in character, are made up chiefly of fragmentary materials, and are occasionally even slightly calcareous,

They are much like some of the Lower Silurian flags in Wales, and are in no degree more highly altered than the majority of those rocks, especially in the more disturbed districts. About three miles to the east of Glyn Laggan these beds die out, or at least are lost, and the Lewisian rocks, fundamental gneiss, hornblende-schists, and mica-schists, such as those described on the east of Loch Maree, again come to the surface; and the whole of the remainder of the section consists of these last rocks, the great mountains Ben Fyn, Mulart, and others being entirely made up of these rocks without a vestige of the unaltered beds reappearing there. Of the gneiss, hornblende-schists, and mica-schists which compose these mountains, it need only be said that, on comparison with others from Loch Maree, Gaerloch, &c., it is impossible to recognize any difference in them, the metamorphism being in each case identical in character, and garnets and other crystals occur in them in equal abundance. The strike of the beds at Ben Fyn he found also to be almost identical with that of those on the west coast, the dip being either to the N.E. or E., and seldom if ever south of that point. He also found these rocks, and with a similar strike, in the low ground in Glyn Docherty, near the road to Auchnasheen; and there the Silurian beds are seen resting unconformably upon them. From this the author believes that the Cambrian and Silurian beds are contained in a basin or depression formed of the older rocks, being, however, now altered in their dip and position by slight faults and some folding which has taken place since they were deposited.

3. "On the Triassic Rocks of Normandy and their Environments." By W. A. E. Ussher, Esq., F.G.S.

The author stated that his investigations were confined to the provinces of Calvados and La Manche, more especially the latter. Having briefly alluded to the physical areas of the Bocage and Cotentin, he proceeded to show that whilst the Secondary rocks were confined to the Cotentin, the presence of several Palæozoic inliers proved that they were of no great thickness. He then briefly described several sections illustrative of observations made by him in walking over the Triassic districts of Valognes, Montebourg, and Carentan, and of Bayeux in Calvados. The result arrived at (as far as possible, despite the presence of drift concealing the Trias almost everywhere) was that the Norman Trias is composed of gravels, sands and sandstones, and marls. The gravels replace and give place to sand and sandstone; but the position of the marls could only be distinctly ascertained near Carentan, where they underlie sandstones. The gravels and sands either directly underlie the Infra-lias, or are separated from it by a thin bed of marl. The Norman Trias can scarcely exceed 200 feet in thickness.

The author then briefly enumerated the Palæozoic rocks of the Bocage, and summed up the results of his investigations in the following conclusions:—

First, that the Triassic rocks of Normandy are the south-easterly prolongation of the Triassic area of Somerset and Devon.

Secondly, that Upper Keuper deposits are alone represented in Normandy.

Thirdly, that fragments of the Palæozoic rocks of what is now Normandy were never incorporated in the Triassic rocks of Devon.

Fourthly, that the constitution of the coasts of Normandy, Devon, and Cornwall is such as to justify a belief that varieties of Cambrian, Silurian, Devonian, and Granitic rocks formed the bed of the Triassic waters in the area now occupied by the English Channel, and that to these sources fragments foreign to the Devonshire soil found in the Triassic beds on the South-Devon coast are to be attributed.

4. "On Foyaite, an Elæolitic Syenite occurring in Portugal." By C. P. Sheibner, Esq., Ph.D., F.G.S. Communicated by Prof. T. M'Kenny Hughes, M.A., F.G.S.

The name foyaite is derived from Mount Foya, in the south of Portugal. This rock occurs intrusive in Devonian grauwacke in the ancient province of Algarve, where it forms two dome-shaped hills, the Foya and the Picota, rising respectively to 2968 feet and 2410 feet. The texture of the rock varies from fine- to coarse-grained, and is sometimes porphyritic. An almost compact variety occurs cutting the coarser rock in dykes and veins. The coarser rock occurs mainly on the southern slopes, where, however, the adjoining grauwacke is less altered than elsewhere. The *massif* is also cut by intrusive veins of phonolite and basalt of Tertiary age. Much rock has probably been removed from the district by denudation.

Macroscopically foyaite consists of orthoclase, elæolite, and greenish hornblende. Orthoclase with imbedded elæolite occurs porphyritically. A lens shows titanite, biotite, magnetite, and pyrite to be accessories. Microscopically examined, the above constituents are seen to be present, and exhibit considerable variety in their mode of occurrence, together with nosean and sodalite as characteristic accessories, and occasional plagioclase (recognized as oligoclase), muscovite, hæmatite, and apatite. The elæolite is irregular in outline; nosean and sodalite are often associated with and imbedded in it. The latter minerals are associated and intergrown. Their mode of occurrence and the tests for their presence are described in detail. Hornblende and augite occur in foyaite in about equal quantities, associated and intergrown. These also are fully described, as well as the characteristics of the other accessories. Analyses of the elæolite and the foyaite are given. The author concludes by pointing out the close resemblance of the rock to ditroite, miascite, and certain syenites of Brevig and Cape Verd, stating that on this account there is no need of a special group of foyaites.

XXII. *Intelligence and Miscellaneous Articles.*

PHOTOGRAPHY AT THE LEAST-REFRANGIBLE END OF THE SOLAR SPECTRUM. BY CAPT. ABNEY, R.E., F.R.S.

NEARLY two years ago I had the honour of reading a preliminary note on photographing the least-refrangible end of the spectrum; and it seems that the time has come when I ought to redeem the promise implied by a "preliminary note" and enter further into the subject. Last year, owing to a change in residence, I was unable to pursue the subject with any degree of activity; but during this last winter and the present spring I have made fair progress in my researches, the results of which I lay before the meeting. These results are principally photographs themselves; and the first to which I shall call attention is one taken through three prisms of dense flint glass, each of which had a vertical angle of 62°. They were placed at the angle of minimum deviation of B, and kept so. The focal lengths of the collimator and camera were 18 inches and 2 feet respectively; and a condensing lens of 6 feet focus was employed to collect the light, the middle of the collimating lens alone being filled with solar rays. In front of the slit was placed a plate of orange glass, in order to cut off the suffused blue rays, which experience had previously taught me were inimical to the production of good negatives, owing to the light dispersed in the prisms themselves. It will be noticed to what an enormous distance below A the impressions of the bands in the ultra red are to found. If the wave-lengths be used as abscissæ and the measured distances of the known lines be used as ordinates, it will be found, if the waves be completed by hand, that a wave-length of not less than 10,400 tenth metres is impressed. Roughly speaking, A is 7600 tenth metres, and D 5900 tenth metres—by which it will be seen that I was within the mark when I announced that I had obtained photographs as much below A as D was above it. Now this negative, though interesting as a feat in photography, has no practical scientific value, as the ultra red is so tremendously compressed that the absolute wave-lengths could not be obtained from it.

About the time I read my last paper, Captain Tupman kindly lent me a speculum-metal grating, by Rutherford, having about 8600 lines to the inch: but it was only lately, after removing its glass covering, that I was fully able to appreciate its value. In all gratings the red, or rather the ultra red, of the first order is overlapped by the ultra violet and violet of the second order, and the higher the order the more overlap there is. To remedy this, which was a defect for the purpose for which I required it, I placed before the slit of the collimator red glass, which completely cut off the yellow and only allowed a little of the green to pass. The prisms were replaced by the grating, and a photograph of the

second order of the spectrum was taken by it. Theoretically speaking, the time necessary for taking a photograph of the first order is $\frac{1}{8}$ that for taking one of the second, and $\frac{1}{27}$ for that of the third. Practically this is not quite true, for reasons which it is unnecessary to enter into here. This induced me to think that if I could get a grating with double the number of lines to the inch, its first order would give me the same dispersion as the second order of the grating I was using, and at the same time the exposure ought to be more than halved. Mr. Lockyer kindly lent me such a grating; and the third photograph was taken by it. You will see that it gives the lines from C to A almost perfectly. A positive copy of this photograph I sent to Professor Piazzi Smyth, as he drew the map of this region of the spectrum, which he published in the last volume of the 'Edinburgh Astronomical Observations;' and I cannot do better than quote his words regarding the accuracy of the photograph:—"As the size of the glasses approximated closely to my standard-camera size, 2·25 inches × 4·25 inches, I was able at once to view them with any magnifying power in a compound microscope long since arranged for such things; and the effect was astounding. I almost thought I was back again in Lisbon, viewing the Sun's spectrum itself as I used to see it."

This acknowledgment of the value of the photograph was particularly gratifying, as it came from an astronomer who had paid special attention to this particular part of the spectrum.

The next plate shows the region of the ultra red as taken with the same grating. The most conspicuous group has a wave-length of about 8400 tenth metres; and the extreme line visible has a wave-length of about 9200 tenth metres. At this point the closer-ruled grating seemed to fail me, and I could not get beyond this point. It then struck me that the glass on which the lines were ruled might absorb the rays beyond (for I must explain that this grating was ruled on glass and silvered at the back), or that the red glass might absorb them. I then reverted to the first grating and adopted a different method of proceeding.

This was Fraunhofer's method, to which I was practically new—though I believe I have been credited (though inaccurately) with using it for other experiments; and after various attempts the following is an outline of the arrangement adopted. The slit was placed horizontally; a prism of 60° occupied a position at the end of the collimator next the lens; the reflection-grating then received the rays and reflected them into the camera-lens, the camera being tilted at an angle to make them fall on the sensitive plate. To show what sort of effect can be got, I exhibit a photograph in which from the first to the fourth order of spectra were impressed on the same plate in a small camera. When this

method was applied to the larger apparatus I got further into the ultra red than I had done before: and such a plate you see before you: the wave-length of the last line of my group of lines visible on the plate is about 10,300 tenth metres. From indications on other plates I am inclined to think that we may get as far as 12,000 tenth metres, which it will be admitted is a tolerable distance to travel along the invisible spectrum. In these ultra regions the lines are faint, but perfectly measurable under a moderate magnifying-power; and I have therefore proposed to myself to make a map of the ultra red. For the purpose I propose to use the overlapping of the higher order of the spectrum over that used. By cutting off the red in one case and using half the length of the slit for one exposure, and then by cutting off the blue and using the remaining half of the slit for the other, we shall have one spectrum over the other. The wave-lengths of the most-refrangible rays are known; and since the dispersion of the higher order is double that of the one below it, the wave-lengths of the latter can be accurately ascertained. When once a scale is obtained, the greatest difficulty will have vanished.

Now as to the process. My object has been to weight the molecules of silver bromide that they may absorb the red rays. With ordinary silver bromide the film allows these very rays to pass through, whilst a blue absorption takes place. In other words, my endeavour has been to find a heavier molecule of a sensitive salt, which shall answer to the swing of the waves of the red and ultra-red rays. This I first accomplished (as I stated at the time) by adding resins to the silver salt and forming what I may call a bromo-resinate of silver. But I am happy to say that I have secured the same end by, I believe, doubling the molecule of the silver bromide. Now this doubling the molecule is a matter of manipulation more than of chemical knowldge: and I might describe the process in detail, as I have already done in papers I have published, and yet the double molecule would not be obtained unless careful manipulation were attended to—manipulation easy to follow when seen, but difficult to follow from any description. I should therefore prefer to teach practically any one who is acquainted with silver-bromide-emulsion-making, rather than allow him to be misled by what must be imperfect directions.

I exhibit two films, both prepared with sensitive emulsion which is composed of exactly similar ingredients, viz. pure silver bromide. You will at once note the difference in colour of the light transmitted. The one which is sensitive to the red and ultra red is of blue tint: the other is orange. You will see that the blue tint would appear to be due to a physical arrangement of the molecules; for if a part of the film be rubbed you will see that it changes to a ruddy tint, passing through an emerald-green stage.

We have it so on the screen. Whether it be right or wrong in regard to other matter, I am convinced that in silver bromide we have the possible existence of two sizes of molecules. In the blue film we have the presence of both; when only the larger size is present we shall have a compound which is very much more sensitive to the lower end of the spectrum than it is to the upper. Allow me to say that these views are not original, except in so far as they are applied to this subject. Whether they are correct or not, they have formed a good working hypothesis which has led me to the results obtained.

Before closing I must refer to the comparative lengths of exposure required for these photographs. My impression is that at A the exposure required is about 25 times that required for G, in a fairly bright sun at midday; for the ultra red, as far as I have gone, I should say about 35 times. These are only approximations, but still will enable you to form some idea of the sensitiveness. I show you a photograph taken about 4 P.M. on March 18. The exposure was about 50 seconds. You will see that the red end is as strong as the blue, with the yellow much lacking in density. In other words, the yellow rays are nearly inactive.

The photograph which I showed you of the four orders of the spectrum was taken on April 3, at 2.30. It had an exposure of one minute and a half. The photograph in which the furthest band of lines was seen had an exposure of 12 minutes, on the 8th, at 2.30. The slit was in this case closed as nearly as possible.

In conclusion, I have to remark that in a short time I hope to reduce the exposures considerably. In the course of some investigations, the results of which have just been communicated to the Royal Society, I found that the red rays could oxidize a photographic image as well as form it, and that in an oxidized state it was unable to be developed. If the tendency of the sensitive compound to become oxidized exceeded its tendency to become reduced, no image could be developed. By exposing *in vacuo*, or in a nitrogen atmosphere, I hope to eliminate altogether this oxidizing effect, and so get firmer images.—*Monthly Notices of the Royal Astronomical Society, April* 1878.

ON THE FRICTION OF VAPOURS. BY DR. J. PULUJ.

Friction-experiments with vibrating disks confirm for vapours also the law that the friction is independent of the pressure up to the limit of saturation, and the law of its proportionality to the absolute temperature, which latter law has been experimentally proved by A. v. Obermeyer and the author for more easily compressible gases.

For ether-vapour the calculation gave, within the temperature-

interval 72–36·5 degrees Centigrade,

$$\eta = 0{\cdot}0000689 \,.\, (1 + 0{\cdot}0041575t)^{0{\cdot}94}.$$

The experiments were made on seven vapours, for which the mean lengths of path were calculated and compared with the exponents of refraction determined by Dulong.

The remarkable relation pointed out by Director Stefan, starting from the view that molecules are surrounded by an envelope of æther, has been confirmed in the case of vapours also. He showed in gases that higher exponents of refraction correspond to shorter lengths of path. Unfortunately, the refraction-exponents for only two vapours (the highest hitherto observed) were determined experimentally by Dulong.

The author obtained for

Hydrogen	$l_0 = 0{\cdot}0000151$	$n = 1{\cdot}000158$
Air	0·0000082	1·000294
Bisulphide of carbon..	0·0000029	1·001500
Ether	0·0000022	1·001530

Director Stefan calculated from the coefficients of diffusion that the mean length of path of ether-vapour is 0·0000023, and of bisulphide of carbon 0·0000032, with which values the above are in very good accordance.

Finally, for the vapours examined the proportional numbers of the molecular volumes were calculated from the friction-constant, according to the formula deduced by Lothar Meyer,

$$\frac{v_1}{v_2} = \left(\sqrt[4]{\frac{m_1}{m_2}} \cdot \sqrt{\frac{\eta_2}{\eta_1}} \right)^3,$$

in which m_1, m_2 are molecular weights, and η_1, η_2 the friction-constants of two gaseous bodies. If the molecular volume of hydrogen v_2 be taken as unity, we obtain for the molecular volume of the vapours examined the numbers in the third column of the Table.

Vapours.	Composition.	$\frac{v_1}{v_2}$.	v_1.	
			From friction.	After Kopp.
Water	H_2O	4·9	22·9	18·8
Bisulphide of carbon...	CS_2	14·0	65·5	62·3
Chloroform	$CHCl_3$	18·6	87·1	84·9
Alcohol	C_2H_6O	11·3	52·9	62·8
Acetone	C_3H_6O	16·4	76·8	78·2
Benzol	C_6H_6	21·2	99·2	99·0
Ether	$C_4H_{10}O$	21·6	101·1	105·8

By means of the molecular volumes v_1, calculated after Kopp from the densities, the molecular volume of hydrogen was de-

termined, according to the above formula, from the friction-constant, to be 4·7.

By multiplying the proportional numbers $\frac{v_1}{v_2}$ by 4·7, the values of v in the penultimate column are obtained, which agree with the numbers in the last column, calculated by Kopp, as well as can in general be expected in experiments of this sort.

The molecular volume of free hydrogen, 4·7, is less by more than half than 11·0, the value calculated from its liquid compounds. The author endeavours to account for this in the following manner. If molecules are spheres surrounded with envelopes of æther of variable density, two such molecules, on central impact taking place, will probably act on each other as soon as they arrive at a distance from one another equal to the sum of the radii of the *actual* sphere of action. The action lasts until the *vis viva* is reduced to zero and reversed. Molecules with greater velocities, in the gaseous state, will approach nearer to one another, and reciprocally penetrate with their æther envelopes more deeply than molecules with lower velocities, in the liquid state. Hence in the former case the radius of the *apparent* sphere of action, consequently the molecular volume, must be smaller than in the latter.

To the same assumption of a variable sphere of action we are conducted, as first remarked by Director Stefan, by the fact that the friction-constant is proportional not to the square root of the absolute temperature, but to another power of the same, which, according to the experiments of A. v. Obermeyer and the author, is greater than $\frac{1}{2}$ and at the highest is $=1$.

A second basis of explanation may, according to the view of Lothar Meyer, lie in the circumstance that, in determining the molecular volume from the density of the liquid compounds, with them the empty space is measured which is open to the atoms for their motions, while, from the magnitude of the obstruction which one particle constitutes for another, only the volume of the gas particles themselves is determined.

Experiments with air under very low pressures led to this result —that while the pressure diminished from 754 to 0·03 millim. the friction-constant became less by only about one half of its initial value, from which it may be seen how *proportionally great* must be the quantity of gas which remains in a very good vacuum, since it can convey quantities of motion so considerable. This is in excellent accordance with the kinetic theory of gases, according to which, in a cubic centimetre of air of one millionth of an atmosphere pressure, nineteen billion molecules are still present.—*Kaiserliche Akademie der Wissenschaften in Wien, math.-naturw. Classe*, July 1, 1878.

ON THE DEPOLARIZATION OF THE ELECTRODES BY THE SOLUTIONS. BY M. LIPPMANN.

It has long been known that certain salts possess a depolarizing property. The first pile with a constant current, constructed in

1829 by M. Becquerel, owes its constancy to the employment of sulphate of copper; the sulphates of zinc and cadmium have been made use of by MM. du Bois-Reymond and J. Regnault for the construction of impolarizable electrodes and constant elements. Notwithstanding the importance of its applications, this property appears to have been but little studied. The experiments I am about to describe have made evident an essential condition of the phenomenon.

It is the following: in order that the electrode may be depolarized, it must be formed of the same metal as is contained in the solution. Thus copper is the only metal which becomes depolarized in sulphate of copper, while gold, silver, and platinum are polarized in that solution. Inversely, copper polarizes in sulphate of zinc, cobalt, &c. A salt depolarizes only its own metal. To make the experiment, operating for example on sulphate of copper and platinum, two strips of platinum are to be immersed in the liquid, and put into communication with the poles of a capillary electrometer. The mercury column is then at zero. A feeble current is then caused to pass into the liquid so as to employ one of the strips as negative or exit electrode. The electrometer shows a deflection, which remains even after the interruption of the current, thus proving that polarization is produced in the sulphate of copper, as it might have been produced in pure or acidulated water. For the same reason, a couple formed of strips of copper and platinum dipping in sulphate of copper furnishes only a current of brief duration, the platinum receiving, owing to its polarization, an electromotive force equal and opposite to that of the copper. One may even go further and communicate to the platinum, by means of an exterior pile, an electromotive force superior to that of the copper, so that then the platinum will behave like a more negative, more oxidizable metal than copper.

Similar experiments have been made with strips and solutions of silver, mercury, lead, cobalt, and zinc.

An application readily presents itself. Since the property of depolarizing a metal belongs exclusively to its salts, it permits us to detect the presence of that metal in a solution. Taking copper as an example, if we dip into the liquid to be tested a copper wire which we use for the negative electrode of a feeble current, it will be polarized if there is no dissolved copper, it will not be polarized if the solution contains $\frac{1}{3000}$ of sulphate of copper. It is possible, therefore, thus to detect the presence of copper in a mixture of metallic salts. With a silver wire we can in the same way test for silver. The delicacy of this electric process appears to be still greater for silver than for copper; but it has not yet been measured.—*Comptes Rendus de l'Académie des Sciences*, June 24, 1878, tome lxxxvi. pp. 1540, 1541.

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XXIII. *Recent Researches in Solar Chemistry.*

By J. N. LOCKYER, *F.R.S.**

THE work which is now being done in the various new fields opened up in connexion with solar studies may be conveniently divided into three perfectly distinct branches. We have, first, that extremely important branch which has for its result the complete determination of the position of every thing which happens on the Sun. This, of course, includes a complete cataloguing of the spots on the sun which have been observed time out of mind, and also of those solar prominences the means of observing which have not been so long within our reach. It is of the highest importance that these data should be accumulated, more especially because it has been determined that both in the case of spots and prominences there are distinct cycles, which may in the future be very much fuller of meaning to us than they seem to be at present.

This brings me to refer to the second branch of the work; and it is this:—These various cycles of the spots and prominences have long occupied the attention both of meteorologists and magneticians; and one of the most interesting fields of modern inquiry, a field in which very considerable activity has been displayed in the last few years, is one which seeks to connect these various indications of changes in the sun with changes in our own atmosphere. The sun, of course, is the only variable that

* Communicated by the Physical Society, May 11, 1878.

we have. Taking the old view of the elements, we have fire represented by our sun, variable if our sun is variable. Earth, air, and water, in this planet of ours we must recognize as constants. From this point of view, therefore, it is not at all to be wondered at that both magneticians and meteorologists should have already traced home to solar changes a great many of the changes with which we are more familiar. This second branch of work depends obviously upon the work done in the first, which has to do with the number (the increasing or decreasing number) of the spots and prominences, and the variations of the positions which these phenomena occupy on the surface of the sun. As a result of this work, then, we shall have a complete cataloguing of every thing on the sun, and a complete comparison of every thing on the sun with every meteorological phenomenon which is changeable in our planet.

When we come to the third branch of the work, the newest branch, things are not in such a good condition. The workers are too few ; and one of the objects of any one who is interested in this kind of knowledge at the present moment must be to see if he cannot induce other workers to come into the field. The attempt to investigate the chemistry of the sun, even independently of the physical problems which are, and indeed must be, connected with chemical questions, is an attempt almost to do the impossible unless a very considerable amount of time and a very considerable number of men be engaged upon the work. If we can get as many workers taking up various questions dealing with the chemistry of the sun as we find already in other branches, I think we may be certain that the future advance of our knowledge of the sun will be associated with a future advance of very many problems which at the present moment seem absolutely disconnected from it.

I have today to limit myself to this chemical branch of the inquiry ; and first let me begin by referring to the characteristics of the more recent work with which I have to deal. Here, as in other branches of physical and chemical inquiry, advance depends largely upon the improved methods which all branches of the science are now placing at the disposal of all others. Our knowledge of the chemical nature of the sun is now being as much advanced by photography, for instance, as that descriptive work of which I spoke in the first instance (which deals with the chronicling and location of the various phenomena) has, in its turn, been advanced by the aid of photography. I do not know whether the magnificent results recently obtained by Dr. Janssen have been brought before this Society ; but the increase in photographic power recently secured by Dr. Janssen is one which was absolutely undreamt

of only a few years ago. It is now possible to record every change which goes on on the sun down to a region so small that one hardly likes to challenge belief by mentioning it. Changes over regions embracing under one second of angular magnitude in the centre of the sun's disk can now be faithfully recorded and watched from hour to hour.

One of the advantages which has come from the introduction of the new apparatus has been the possibility of making maps, on a very large scale, of the solar lines and of the metallic lines which have to be compared with them. Thanks to the great generosity of Mr. Rutherfurd, who is making the most magnificent refraction-gratings which have ever been seen, and who is spreading them broadcast among all workers in science, one has now easy means of obtaining with inexpensive apparatus a spectrum of the sun, and of mapping it on such a scale that the full magnification of the fine line of light which is allowed to come through the slit will form a spectrum the half of a furlong long : an entire spectrum on this scale, when complete (as I hope it some day will be, though certainly not in our time) from the ultra-violet, already mapped by Mascart and Cornu, to the ultra-red, which has quite recently for the first time been brought under our ken by Captain Abney, will be 315 feet long. This is a considerable scale to apply to the investigation of these problems; but recent work has shown that, gigantic as the scale is, it is really not beyond what is required for honest patient work. I have already had an opportunity of bringing before the Physical Society several of the methods in use for comparing the spectra of the various elementary bodies with that of the sun. It is not, therefore, necessary now to refer to them. There are, however, others of recent application which are of very considerable importance.

When, instead of inquiring into the coincidence of the metallic lines, we wish to determine the coincidence of the lines due to various gases, the method hitherto employed has been to enclose the gases in Geissler tubes, to reduce their pressure, and in that way to fine down the lines. The importance of this apparently small matter can be very well demonstrated by an experiment easily arranged in an electric lamp, which sodium enables us to perform without any great difficulty. The point of this experiment is that, if we vary the density of any vapour, we vary sometimes to a very considerable extent the thickness and intensity of the lines. I am about to throw the spectrum on a small screen which I have behind the lamp ; and I hope I shall succeed in rendering the phenomena visible. I want you to observe the variation in the thickness of the reversed

line of sodium. Every turn of the screw which raises or lowers the upper pole, enables me to vary to a very considerable extent indeed the thickness of that absorption-line.

Now the way in which that has been managed is very simple. The only arrangement required is one which shall enable me at will to vary the density of the sodium-vapour. When I make the sodium-vapour as dense as possible, then the line is very thick. When I make it much less dense, the line becomes thinner. If the spectrum on the screen had been a gas-spectrum (supposing it were possible to exhibit a gas-spectrum to an audience), the exact equivalent of that experiment would have been, that the gaseous spectrum at atmosphere pressure would have given us most of the lines as thick as the sodium-line was at its thickest; while if by any possibility we could have rendered the phenomena visible while the pressure was being reduced, as the pressure of the gas was reduced the line would thin. Now there are very great objections to the using of Geissler tubes. One very valid objection is that the gas becomes much less luminous as its pressure is reduced.

Here is a method which is excellent in this way, that it enables all the work connected with gaseous spectra to be done at atmospheric pressure, and we get the line down as thin as we choose, *not by reducing the pressure*, but by *reducing the quantity of gas in a mixture*. If we take, for instance, a spark in ordinary atmospheric air and observe its spectrum, we find the lines of the constituents of atmospheric air considerably thick; but if I wish to reduce the lines, say of oxygen, down to a considerable fineness so that I can photograph its lines (these should be fine, in order to enable me to determine their absolute position; to accomplish this) the spark is made to pass in a glass vessel with two adits and one exit tube. If I wish to observe the oxygen-lines fine, I flood the vessel with nitrogen so that, say, there is only 1 per cent. of oxygen present, and observe the current between the enclosed electrodes. If I wish to observe nitrogen-lines fine, I flood it with oxygen, so that there is only 1 per cent. of nitrogen present. In this way, by merely making an admixture in which the gas to be observed is quantitatively reduced, so that the lines which we wish to investigate are just visible in their thinnest state, we have a perfect means of doing this without any apparatus depending on the use of low pressures; and those who have worked most with Geissler tubes will appreciate the very great simplicity of work which is thus introduced.

Another important application of spectroscopic theory recently applied to the investigation of the chemistry of the sun is this:—Assume that the spectrum of any substance is not a

pure spectrum of that substance, but of that substance as it generally exists in an impure state.

The spectrum will be found rich in lines; and when very considerable care is employed, one may go away with the idea that in iron, for instance, all the lines which are observed in the spectrum of iron coincident with Fraunhofer lines represent coincidences in the case of each line with iron in the sun and iron in our laboratory. But the more the work is carried on, the more one finds that the complex spectra which are observed are really much more simple when all the impurities are taken into account.

In the region of the solar spectrum, for instance, recorded on the map exhibited we have a great many iron-lines; but before the method of determining impurities was utilized, the spectrum was very much richer than it is at present; possibly one fourth of the lines have been withdrawn. In every specimen of iron which has been used in this work the lines of calcium, aluminium, and some of the lines of manganese and cobalt have been represented; and no chemist will wonder at this result. But there is a very curious thing which chemists, I think, will wonder at. In this part of the spectrum there were two lines which, by their thickness both in the solar and iron spectra, seemed undoubtedly to belong to iron; but further inquiry led to this extraordinary result—that one of these lines in all probability has its origin in the vibration of molecules of tungsten, the other being probably a line of molybdenum. Glucinum is another metal which may be referred to in this connexion; and it would appear that it is almost impossible to get a specimen of iron which does not contain, not only calcium and aluminium, but others which we consider rare metals on the earth, such as tungsten, molybdenum, and glucinum.

A few years ago, taking the work of Kirchhoff, Bunsen, Ångström, and Thalén into consideration, and connecting it, so far as one could connect it, with those ideas of which recent eclipses have been so fruitful, our chemical view of the sun's atmosphere was one something like this:—We had, let us say, first of all an enormous shell of some gas, probably lighter than hydrogen, about which we know absolutely nothing, because at present none of it has been found here; inside this we have another shell, of hydrogen; inside this we have another shell, of calcium, another of magnesium, another of sodium, and then a complex shell the section of which has been called the reversing layer, in which we get all the metals of the iron group plus such other metals as cadmium, manganese, titanium, barium, and so on. The solar atmosphere, then, from top to bottom, consisted, it was imagined, of a series of shells,

the shells being due not to the outside substance existing only outside, but to the outside substance extending to the bottom of the sun's atmosphere, and finding in it at a certain height another shell, which again formed another shell inside it, and so on; so that the composition of the solar atmosphere as one went down into it, got more and more complex: nothing was left behind; but a great many things were added.

The recent work, so far as I am acquainted with it, has not in any way upset that notion; but what it has done has been to add a considerable number of new elements to this reversing layer. Instead of consisting of 14 elements, as it was then found to do, it may be, I think, pretty definitely accepted now to consist of about thirty.

The metals considered to be solar as the result of the labours of Kirchhoff, Ångström, and Thalén together with the considerations brought forward regarding the length of the lines, were as follows:—

Na	Fe	Ca	Mg	Ni
Ba	Cu	Zn	Cr	Co
H	Mn	Ti	Al	

Those more recently added, with the evidence by which their existence in the solar atmosphere is rendered probable, are as follows (in the Tables, pp. 167–169).

It is important to bear in mind that the lines recorded in these Tables are in most cases the very longest visible in the photographic region of the respective spectra; in some cases they are limited to the region 39–40, which I have more especially studied; so that the fact of their being reversed in the solar spectrum must be considered the strongest evidence obtainable in favour of the existence in the sun of the metals to which they belong, pending the complete investigation of their spectra.

Where, however, there is only one line, as with Li, Rb, &c., the presence of these metals in the sun's reversing layer can, for the present, only be said to be probable. Neither must it be forgotten that, in addition to the long lines which a spectrum may contain in the red, yellow, or orange, long lines may exist in the hitherto unexplored ultra-violet region; so that the necessity for waiting for further evidence before deciding finally upon the presence or absence of such metals in the sun will be rendered obvious.

It will be thought remarkable that, if the long lines of such metals as lithium and rubidium are found in the photographic region of the spectrum, the long lines Li W.L. 6705, Rb W.L. 6205 and 6296 should have escaped detection.

Metals the presence of which in the Sun is confirmed.

Name of metal.	Approximate W.L. of lines reversed in the solar spectrum.	Observations.	By whom previously mapped.	Particulars from Thalén's Tables.		
				Metal.	Wave-length.	Intensity. 1 = brightest.
Strontium	4029·60	Absent from spectre normal, near long Mn line	New line.			
	4076·77	A line at 4076·9 in spectre normal assigned to Ca	Thalén	Sr	4078·5	1
				Ca	4077·0	3
	4215·00	In spectre normal assigned to Ca, W.L. 4215·40	Thalén	Sr	4215·3	1
				Ca	4215·3	1
	4607·50	A line at 4604·5 in spectre normal assigned to Ca	Thalén	Sr	4607·5	1
				Ca	4607·5	4
Lead	4019·28	Absent from spectre normal	New line.			
	4056·80	An unassigned line near required position (W.L. 4057·25) in spectre normal	Thalén	Pb	4058·0	4
	4061·25	Absent from spectre normal	Thalén	Pb	4062·5	4
Cadmium	4677·00	An unassigned line at W.L. 4676·5 in spectre normal	Thalén, Kirchhoff	Cd	4676·8	1
	4799·00	An unassigned line at W.L. 4798·9 in spectre normal	Thalén, Kirchhoff	Cd	4799·0	1
Potassium	4042·75	Absent from spectre normal	New line.			
	4046·28	Absent from spectre normal	New line.			
Cerium	3928·7	Absent from spectre normal	New line.			
	4012·0	Absent from spectre normal	New line.			
Uranium	3931·0	Absent from spectre normal	New line.			
	3943·0	Close to Al line	New line.			
	3965·8	Absent from spectre normal	New line.			

Metals probably present in the Sun.

Name of metal.	Approximate W.L. of lines reversed in the solar spectrum.	Observations.	By whom previously mapped.	Particulars from Thalén's Tables.		
				Metal.	Wave-length.	Intensity. 1 = brightest.
Vanadium	3901·3		New line.			
	3909·3		New line.			
	3989·65		New line.			
	3992·5		New line.			
	3997·9		New line.			
	4379·0	Assigned by Ångström to Ca	Thalén	Ca	4379·1	4
				Va	4379·0	1
	4384·0	Absent from Ångström's map	Thalén	Va	4384·0	1
	4389·0	Assigned by Ångström to Ca	Thalén	Ca	4389·4	4
				Va	4389·0	2
	4407·5	Assigned by Ångström to Ca	Thalén	Ca	4407·0	5
				Va	4407·5	1
Palladium	3893·0		New line.			
	3958·0		New line.			
	4787·0	Assigned by Ångström to Fe	Thalén	Pd	4787·0	3
				Fe	4785·8	5
	4817·0	Not allocated by Ångström	Thalén	Pd	4817·0	3
	4874·0	A line near required position assigned by Ångström to Fe	Thalén	Pd	4874·0	3
Molybdenum	3902·0	Very near Fe line	New line.			
	4576·0	Not allocated by Ångström	New line.			
	4706·0	Assigned by Ångström to Fe	Thalén	Mo	4706·5	4
				Fe	4706·5	5
	4730·0	Assigned by Ångström to Fe	Thalén	Mo	4730·5	4
	4818·0?	Absent from Ångström's map	Thalén	Mo	4818·0	4
	4829·0?	Line near assigned by Ångström to Ni	Thalén	Mo	4829·5	4
				Ni	4828·4	5
				Ni	4830·2	5

Metals probably present in the Sun (*continued*).

Name of metal.	Approximate W.L. of lines reversed in the solar spectrum.	Observations.	By whom previously mapped.	Particulars from Thalén's Tables.		
				Metal.	Wave-length.	Intensity. 1=brightest.
Indium	4101·0	Apparently coincident with Fe line in solar spectrum	Thalén	In	4101·0	1
	4509·0	Absent from Ångström's map	Thalén	In	4509·5	1
Lithium	4603·0	The line in Ångström's map is placed at W.L. 4601·7 and no metal assigned	Thalén	Li	4602·7	1
Rubidium	4202·0	The line in Ångström's map is placed at 4201·0 and made winging the adjacent Fe line	Thalén	Rb	4202·0	2
Cæsium	4554·9	Assigned by Ångström to Fe; no line in Thalén in this position	New line.			
	4592·0	Absent from Ångström's map	New line.			
Bismuth	4722·0	Absent from Ångström's map	Thalén	Bi	4722·0	1
Tin	4524·0	Not in spectre normal, a neighbouring Bi line at 4524·4 is shown	Thalén	Sn	4524·0	1
Lanthanum	3948·2	Not in spectre normal	New line.			
	3988·0	Not in spectre normal	New line.			
	3995·04	Not in spectre normal	New line.			
Glucinum	3904·75	Not in spectre normal	New line.			
Yttrium or Erbium	3949·6	Not in spectre normal	New line.			
	3981·8	Not in spectre normal	New line.			

To this it may be replied that, although these red lines may be apparently the brightest to the eye, it by no means follows they are the longest, since they are situated in a part of the spectrum which affects the visual organ more strongly than the photographic region does. It is possible also that the reasoning I have lately used in a paper communicated to the Royal Society, on the spectrum of calcium, may be applied in these cases.

Since a sensitized film is affected by some rays more strongly than by others, in determining the lengths of lines from a photograph it is not fair to compare together portions of the spectrum separated by too great an interval.

Furthermore, the fact of these red lines having been overlooked in the solar spectrum is not conclusive proof of their absence, inasmuch as this portion of the spectrum is both brighter and less refrangible, and a greater degree of dispersion would be necessary when prisms are employed to render visible faint dark lines which are easily detected in the photographic region.

At present, then, out of the fifty-one metals with which we are acquainted here, more than thirty are known to exist in the sun with more or less certitude. Now it was a very remarkable thing that although such metalloids as carbon and sulphur, iodine, bromine, and the like, had been very diligently searched for, no trace whatever had been found of them, giving any evidence that they existed together with the metals in these zones (these shells) to which I have referred.

Some years ago evidence was brought forward of the possible existence of the metalloids as a group outside the metals; and the evidence for this suggestion was of the following nature:—Independently of any questions connected with solar physics, I think all students of science now agree that the vapours of the various elementary bodies exist in different molecular states; if these different molecular states are studied by means of the spectroscope, perfectly different spectroscopic phenomena present themselves. If we use a large coil, we can drive every chemical substance with which we are acquainted, including carbon and silicon, into a molecular grouping competent to give us what is called a line spectrum, the spectrum with which we are most familiar when we use metals or salts of metals in the electric arc.

If, however, other conditions are fulfilled; if these bodies are not so roughly handled—if, in other words, we employ a lower degree of heat, or if we use electricity so that we get quantity instead of tension, then these line spectra die away altogether, and we have a spectrum, so called, of channelled

spaces or flutings. Perhaps it will be convenient that I should throw one of these spectra on the screen, and point out exactly the difference to which I refer. I will first call attention to a line spectrum. Those lines are due to the vibrations of molecules of calcium and aluminium. The flutings which I now throw on the screen are perfectly different in appearance; in this case they have been produced by the vibrations of carbon at exactly the same temperature at which we get the line spectrum from aluminium and calcium.

Now, while we got these thirty-three metals to give us line spectra coincident with Fraunhofer lines, the only evidence (very doubtful evidence) of the existence of the metalloids in the sun at all, depended on the fact that, in the case of iodine and chlorine, some of the channelled spaces observed in their spectra at a very low temperature were imagined to be traced among the Fraunhofer lines in the spectrum of the sun. It is four years ago since evidence was gathered of a more conclusive kind in the case of carbon. The kind of evidence will be sufficiently indicated by throwing a comparison of the solar and carbon spectra on the screen. Below we have the bright flutings due to carbon-vapour; and above the solar spectrum this photograph includes a part in the ultra violet. When this negative is placed under a magnifying-glass, we find that most of the very delicate lines constituting the fluting in the bright portion have their exact equivalents among the Fraunhofer lines. This is the best-established piece of evidence, so far as I know, which seems to indicate that we have truly some of the metalloids present in the atmosphere of the sun by the coincidence of their spectra with the Fraunhofer lines. Further, carbon at all events exists under such conditions that its molecular structure is very much more complex than that of the metals in the reversing layer; and therefore it is probably withdrawn from the excessive heat of the lower region occupied by the reversing layer, which is competent, as we know from from other considerations, to drive even carbon and silicon into the line-stage, supposing carbon and silicon to be there.

This branch of the work to which I have just referred, a branch which enables us to say that such a temperature must exist in such and such a region of the solar atmosphere, depends, in the main, upon questions raised by the differences between the spectra of certain bodies in the sun and in our laboratories. If, for instance, one wishes to observe the coincidence between, let us say, iron and the sun, iron is placed in the electric lamp; its spectrum is photographed: side by side with it we have the spectrum of the sun also photographed; and, as a rule (I say as a rule; but this is not abso-

lute in the case of such metals as iron), the intensity of the iron-lines which we get in our laboratories is equivalented by the intensity of the so-called iron-lines which we assume to exist in the spectrum of the sun. That is the great argument, in fact, for the existence of iron in the sun. But when we leave the iron group of metals, we find others in which this coincidence, this great similarity of intensity from one end of the spectrum to the other, is very considerably changed. We get in the case of calcium very thick lines of calcium corresponding with very thin lines in the sun, and we get thin lines of calcium corresponding with very thick lines in the sun. In fact, the two thickest lines which have already been mapped in the spectrum of the sun are lines due to calcium. If we photographed the spectrum of calcium with a very weak arc in that electric lamp, they would scarcely be visible at all. If, however, we pass from the tension of the arc to the tension which is obtainable with the use of a very large coil, then we can make the spectrum which we get artificially correspond exactly with the spectrum with which the sun presents us naturally; and the more we increase the tension (the larger the coil and the larger the jar we employ), the more can we make our terrestrial calcium vibrate in harmony, so to speak, with the calcium which occupies a very definite region in the atmosphere of the sun. Now this gives us this very precious teaching:—We know that the vapour of *calcium* occupies such and such a position in the sun; we know that to get the two things in harmony, as I said before, we must employ a very large induction-coil; and we know, again, that if we do employ a large induction-coil, all these beautiful flutings in the *carbon*-spectrum which have been thrown on the screen disappear utterly. That kind of carbon is no longer present in the reaction; but instead of it we have a new kind of carbon which is only competent to give us bright lines. We know, fourthly, that those bright lines do not exist reversed in the spectrum of the sun. Therefore the carbon must exist higher than the calcium, in a region of lower temperature.

In what I have said up to the present moment (and I have just touched very slightly on the physical side of the work, because I believe that in the future it will be most rich in teachings of the kind I have indicated), I must remind you that I have dealt solely with the Fraunhofer lines. Now it is knowledge ten years old, that if we observe the solar spectrum with that considerable dispersion which is now, I think, imperative if we are to do much good with it, there are bright lines in the ordinary solar spectrum side by side with the dark ones.

In a paper communicated to the Royal Society in 1868 I find these words:—"Attention has recently been drawn to certain bright regions in the ordinary spectrum." The position of these bright lines in the ordinary spectrum was then stated, and attention was called, among others, to one between *b* and F. I call especial attention to that line now because the requisite amount of dispersion is now so common that any one, whenever the sun shines, may turn to *b* and see that bright line for himself. It will be found just as much outside the fourth line of *b* as the third line is on the other side of it. This bright line, lying in the most visible part of the spectrum, is exactly similar to many others, some of them in the yellow and some of them in the red. A careful list of these lines was made some years ago; and, I am sorry to say, the list was unfortunately lost by one of my assistants in a Metropolitan Railway-carriage; at all events, enough was said in this and other countries about these bright lines in the years 1869 and 1870 to have given rise, at all events, to the hope that any one interested in solar physics would be perfectly familiar with them. Among other matters which called attention to their existence was a correspondence which took place in the *Comptes Rendus* of the Academy of Sciences in Paris between Father Secchi and another observer in connexion with solar spots. I have remarked that a large dispersion is requisite to see these bright lines, because with a small dispersion bright regions of another kind in the solar spectrum are very obvious. When this small dispersion, however, is changed for a large one, one sees that these bright regions in the solar spectrum are due to the absence of fine lines; and, indeed, if one observes the solar spectrum with considerable dispersion through a cloud which prevents the fine lines from being seen, then there is a very considerable relative diminution in the intensity of some parts of the spectrum, and a very considerable relative increase in others, where these very fine lines are present and absent relatively, so as to give rise to the appearance of a very considerable change indeed in the background of the spectrum.

When, however, a very considerable dispersion is employed and photography is brought into play, if precautions be taken to give sufficient exposure, these bright regions, as opposed to the bright lines, entirely disappear. I have here, by the kindness of two friends, Mr. Rutherfurd and Captain Abney, the means of showing you exactly what I mean. A Rutherfurd grating containing 17,000 lines to the inch has been used as a means of obtaining the spectrum; and the film employed was kindly put on the plate for me by Captain

Abney himself. We find, now, that Mr. Rutherfurd has given us an engine of such enormous power that the fineness of the collodion film is entirely distanced; that is to say, we can get from these perfect gratings spectra so extremely fine and so full of detail, that they can be enlarged until the structure of the ordinary collodion comes in and prevents a fine picture. But if instead of the ordinary collodion process, those which are being worked out with such success by Captain Abney be employed, then it appears that the film is as perfect a thing in its way as the grating is in its way, and one can go on obtaining any magnification one wants.

This is a photograph of the H lines obtained by the grating and film to which I have referred. Between the H and K lines, where the eye sees faintly three lines, there are now nearly a hundred; and that will speak more than any words of mine as to the extreme importance of the introduction of photography in such a research as this. Now here there are no bright lines; but, very conveniently, this next photograph contains one of the bright lines discovered and carefully recorded by Cornu, who has recorded bright lines in the ordinary solar spectrum as well as Hennessy. In exactly the middle of the field now is the bright line recorded as a bright line by Cornu in his map of the blue end of the solar spectrum; but excepting that one bright line, which is much more intense than any other part of the spectrum, bright lines are non-existent.

During the course of last year Dr. Draper, of New York, published the first results of a research which he has undertaken, going over very much the same ground with regard to the metalloids as had been gone over in this country with regard to the metals. Dr. Draper, who has long been known as a most earnest student of science, approached this subject with a wealth of instrumental means almost beyond precedent; and his well-known skill and assiduity, in the course of the two or three years during which his work was carried on, enabled him to accumulate facts of the very greatest importance. I am most anxious to make these preliminary remarks, and to state my very highest respect for Dr. Draper, because in referring to his work I shall have to point out that some of his results are, in my opinion, not yet completely established. Dr. Draper, in the first instance, claims the discovery of the bright lines already referred to, and bases a new theory upon them. It is by no means as a stickler for priority that I regard this as a very great pity, but because I think that, if the very considerable literature touching these bright lines (papers by Young, Cornu, Hennessy, Secchi, and others) had been before Dr. Draper when his paper was written, the necessity for the establishment of a new theory of the solar spectrum, which doubtless

cost him very considerable thought, would probably have been less obvious.

Dr. Draper was so kind as to send me some little time ago a photograph of the solar spectrum confronted with the lines of oxygen ; and the result which this photograph is claimed to show is, that a considerable number of the oxygen-lines are coincident with bright lines in the solar spectrum. I will throw this photograph of Dr. Draper's on the screen, in order that we may have common ground of thought. The lower part of the photograph gives the lines of oxygen ; the middle part gives Dr. Draper's photograph of the sun, and the upper part a photograph of the sun taken in England, which I have put side by side with Dr. Draper's in order that the definition of the two photographs may be contrasted.

On examining the upper photograph with a very considerable magnifying-power, the detail comes out marvellously, and the spectrum between the more marked lines is found to be occupied with extremely fine lines in those regions where Dr. Draper's photograph gives ribbed structure, which, I fear, may not be due to the solar spectrum at all. In the silver-on-glass gratings, one of which Mr. Rutherfurd was so kind as to give me, I find that, in consequence of the grating being ruled on the back surface of the glass and the double transmission of the light through the plate, there is a considerable formation of Talbot bands, and the solar spectrum is in some regions entirely hidden and absolutely transformed. Lines are made to disappear ; lines are apparently produced ; so that if one compares a part of the spectrum taken with one of these silver-on-glass gratings with an ordinary refraction-spectrum, the greatest precaution is requisite. Indeed I think I am not going beyond the mark when I say that the positions of all lines below the third or fourth order of intensity must be received with very great caution indeed when these gratings are employed. So well is this known to Mr. Rutherfurd himself, who prepared these gratings for another purpose, that he is now, with equal generosity, distributing gratings containing the same number of lines to the inch (17,300, or something like that) engraved on speculum-metal in order that these defects may be obviated.

With regard to this work of Dr. Draper's, then, I wish to point out that the photograph in which these comparisons with the oxygen-lines have been made is not one which is competent to settle such an extremely important question. Secondly, upon examining these oxygen-lines, I do not find the coincidences to which he refers with bright solar lines and oxygen-lines in that part of the spectrum with which I am most familiar, for the reason that there are no bright lines whatever in

this portion of the spectrum. I have here enlargements of negatives going nearly the whole length from G to H, one of the regions which are included in this photograph of Dr. Draper's. I have carefully gone over these regions line for line; and in no case do I see any bright line in the sun whatever coincident with any line of oxygen whatever. I cannot profess to have gone over the ground in the ultra violet; but it will appear to me very surprising indeed if, when we go further, when we include the H and K lines which have already been thrown on the screen, that Dr. Draper will find any possible coincidences with bright lines of the sun even there, because, when perfect instrumental conditions are brought into play, no bright line whatever exists in the part of the solar spectrum which is included in this map.

The bright line discovered by Cornu exists outside K; but between the region included in this map and the G lines I find no obvious bright line.

There is an experiment which any member of the Physical Society who possesses a spectroscope with three or four prisms can make for himself. Take the spark in air in an apparatus of the kind to which I have referred, use a comparison prism, flood the air with nitrogen, and in the field of view which includes *b* (and therefore one of the most marked bright lines in the solar spectrum itself) you will find three or four undoubted lines of oxygen. I have made that experiment, which is quite a simple one; and I find no coincidences in this part of the spectrum between any of these oxygen-lines and the undoubted bright lines. I have not tried it yet for the lower parts of the spectrum in the red and yellow, because I hope that Dr. Draper will try for himself.

I do not say that Dr. Draper's alleged discovery is no discovery at all; I say (and I think it is my duty to say it, as I have been occupied in closely allied work for some considerable time) that I do not hold it to be established.

I have no doubt that Dr. Draper will carefully go over his work himself; and I am quite certain that he will be the very first to hail what I have said today with satisfaction, because his desire, I am sure, is the desire of every true man of science, that the truth should prevail. In any case Dr. Draper has begun work in a branch of the chemical inquiry into solar matters which, up to the present time, has been sadly neglected; and we should all be grateful to him on that ground. I have no doubt that he on his side, as I on mine, hopes, as I said before, that the Physical Society of London and the Physical Societies of America will come forward and supply more workers for a branch of science which I am certain in the future will be regarded as one of very considerable importance.

XXIV. *On the Resistance of Telegraphic Electromagnets.* By Oliver Heaviside*.

1. THERE appears to be some uncertainty regarding the proper resistance which electromagnets for signalling-purposes should have—whether a receiving instrument should have a resistance equal to that of the remainder of the circuit, or a half, or a fifth, or some other fixed fraction thereof. Practical experience, especially with high-speed instruments, has shown that the resistances in general use are too high—and that advantage is gained by reducing the resistance of an electromagnetic receiving instrument, employing fewer windings of a thicker wire in place of more windings of a thinner, thereby reducing the self-induction as well. Ohm, as long ago as 1826, showed that the resistance of a galvanometer should equal that of the rest of the circuit in which it is placed, to obtain the maximum magnetic force. When the correction needed on account of the thickness of the insulating covering of the wire is also reckoned, then the thickness of the wire of the galvanometer should be such that the external resistance is to the resistance of the galvanometer-coil as the diameter of the covered wire is to the diameter of the wire itself (Maxwell, vol. ii. p. 321).

Now if, in telegraphic signalling, sufficient time were allowed during every signal (positive, negative, or no current) for the full effect to be produced in the circuit by the electromotive force, or for the current to entirely die away, the above result would hold good also. But such is not the case; for by reason of electrostatic and electromagnetic induction, the current has not time to reach its full strength during every signal. On a land-line, unless it is very long, electromagnetic induction is the principal retarding cause; and it is this case which is here considered.

2. Let there be a simple harmonic variation of electromotive force

$$\mathrm{E}\sin mt,$$

where E and m are constants, and t is the time, in a circuit of resistance R and electromagnetic capacity L. The equation of the current is

$$\mathrm{E}\sin mt=\left(\mathrm{R}+\mathrm{L}\frac{d}{dt}\right)\gamma, \quad . \quad . \quad . \quad . \quad . \quad (1)$$

where γ is the current at time t. The solution of (1) is

$$\gamma=\frac{\mathrm{E}}{\sqrt{\mathrm{R}^2+\mathrm{L}^2m^2}}\sin\left(mt-\tan^{-1}\frac{m\mathrm{L}}{\mathrm{R}}\right),$$

* Communicated by the Author.

neglecting a vanishing term. The amplitude of the current-waves is thus reduced from

$$\frac{E}{R}$$

(what it would be were there no retardation) to

$$\Gamma=\frac{E}{\sqrt{R^2+L^2m^2}}, \quad \ldots\ldots\ldots \quad (2)$$

where Γ signifies the maximum current. Or the current is the same as if the resistance were increased in the ratio of

$$1:\sqrt{1+\left(\frac{Lm}{R}\right)^2}.$$

If Lm is large compared with R, then Γ is small compared with $\frac{E}{R}$, or the diminution of current-strength is large.

Let T be the time of a complete reversal. Then

$$m=\frac{2\pi}{T}.$$

With the Morse code, when $T=\frac{1}{40}$ second, or $m=80\pi$, an automatic transmitter produces 100 words per minute, or a little more. Let $\frac{L}{R}$ be between $\frac{1}{1000}$ and $\frac{1}{10}$ second, then $\frac{Lm}{R}$ is between $\frac{8\pi}{10}$ and 8π, or (say) between 2·5 and 25; therefore the current will be reduced from 2·5 to 25 times.

On telephonic circuits, owing to the great rapidity of the reversals, the reduction in current-strength is great, and is nearly inversely proportional to the pitch of the tone, thus rendering it impossible to reproduce at the receiving end the same quality of sound as is emitted at the sending end, irrespective of mechanical or acoustical difficulties. The second partial tone of any continuous sound will be weakened twice as much as the first, the third thrice as much as the first, and so on, thus producing a general deadness or want of brilliancy.

It also appears from (2) that the resistance of the circuit becomes quite subordinate when $\frac{Lm}{R}$ is large; and it may then be greatly increased without much weakening the current. This is remarkably evident on telephonic circuits. On automatic circuits it has sometimes been found beneficial to introduce resistance-coils at the receiving end. The irregular effects of leakage from neighbouring wires, which mutilate

the proper signals, are reduced in a greater proportion than the proper signals themselves.

3. Now let R and L belong to the electromagnet alone, and R_1 and L_1 be the resistance and electromagnetic capacity of the remainder of the circuit. Then (2) becomes

$$\Gamma = \frac{E}{\sqrt{(R+R_1)^2 + m^2(L+L_1)^2}}. \quad . \quad . \quad . \quad . \quad (3)$$

In solenoidal electromagnets, if n is the number of windings of the wire in unit of length, and the number of layers in unit of thickness, R varies as n^4. L also varies as n^4, while the magnetizing force due to the unit current varies as n^2. Applying this to equation (3), if F is the magnetizing force, F is a maximum, n being variable, when

$$R^2 + L^2m^2 = R_1^2 + L_1^2m^2, \quad . \quad . \quad . \quad . \quad . \quad (4)$$

R_1 and L_1 being considered constant, as belonging to the line. We may write (4) thus,

$$\frac{R}{R_1} = \sqrt{\frac{1+m^2\left(\frac{L_1}{R_1}\right)^2}{1+m^2\left(\frac{L}{R}\right)}}. \quad . \quad . \quad . \quad . \quad . \quad (5)$$

Now $\frac{L_1}{R_1}$ is constant for the same line-wire, whatever its length, since both L_1 and R_1 are proportional to the length of the line. Also $\frac{L}{R}$ is constant for the same coil, if only the diameter of the wire is variable, since both L and R vary as n^4. But the time interval $\frac{L}{R}$ for the electromagnet is in general much greater than the time interval $\frac{L_1}{R_1}$ for the line-wire; whence it follows, by inspection of (5), that R must be much less than R_1 to produce the maximum magnetizing force; and the higher the speed, which is proportional to m, the less should R be.

The calculation of $\frac{L_1}{R_1}$ is easy, since the line-wire is long, straight, and parallel to the earth; but the calculation of $\frac{L}{R}$ is not so easy, owing to the variety of shapes assumed by electromagnets used for telegraphic purposes, with their cores, polepieces, and armatures, which all influence the electromagnetic capacity, though they do not influence the resistance. It is therefore impossible to enunciate a general law, that the

resistance of an electromagnet should be such or such a fraction of the external resistance; for the result will be different, not only for every speed, but for every different construction of the electromagnet.

4. Approximate results are, however, easily obtainable in the case of a solenoidal electromagnet. Let its length be l, external radius x, internal radius y, with an iron core of radius z. Its electromagnetic capacity is

$$\mathrm{L}=\tfrac{2}{3}\pi^2 l n^4(x-y)^2(x^2+2xy+3y^2+24\pi\kappa z^2) \quad . \quad . \quad (6)$$

('Maxwell,' vol. ii. p. 283), where κ is the coefficient of magnetization of the core. Its resistance is

$$\mathrm{R}=\pi\rho l n^4(x^2-y^2), \quad . \quad . \quad . \quad . \quad . \quad (7)$$

where ρ is the resistance of unit of length of wire of unit diameter. Therefore

$$\frac{\mathrm{L}}{\mathrm{R}}=16\pi^2\frac{\kappa}{\rho}\frac{x-y}{x+y}z^2 \quad . \quad . \quad . \quad . \quad . \quad . \quad (8)$$

approximately, by leaving out $x^2+2xy+3y^2$ in (6) as small in comparison with $24\pi\kappa z^2$, which is a large number, unless the core is very small. Let $\kappa=32$; also, if the specific resistance of copper be taken at 1·7 microhm = 1700 c. g. s., then $\rho=1700\times\frac{4}{\pi}$, and

$$\frac{\mathrm{L}}{\mathrm{R}}=2{\cdot}33\frac{x-y}{x+y}z^2 \text{ seconds.} \quad . \quad . \quad . \quad . \quad . \quad (9)$$

5. To determine $\frac{\mathrm{L}_1}{\mathrm{R}_1}$ for the line wire, Maxwell (vol. ii. p. 282) gives the coefficient of self-induction of a straight wire, the circuit being completed by a parallel wire. The same method of calculation is applicable to any number of parallel straight wires, by finding the integral

$$\mathrm{T}=\tfrac{1}{2}\iiint \mathrm{H}w\,dx\,dy\,dz,$$

where T is the kinetic energy of the system, and H, w are the vector-potential and the current at a point, both parallel to the axes of the wires. Thus, for n parallel straight cylindrical wires of length l, conveying currents $\mathrm{C}_1, \mathrm{C}_2, \ldots$, of radii $a_1, a_2, \ldots$, specific magnetic capacities $\mu_1, \mu_2, \ldots$, representing the distance between the centre of two wires m and n by b_{mn} we shall have

$$\left.\begin{aligned}\frac{2\mathrm{T}}{l}&=\tfrac{1}{2}(\mu_1\mathrm{C}_1^2+\mu_2\mathrm{C}_2^2+\ldots)\\&\quad-2\mu_0(\mathrm{C}_1^2\log a_1+\mathrm{C}_2^2\log a_2+\ldots)\\&\quad-4\mu_0(\mathrm{C}_1\mathrm{C}_2\log b_{12}+\mathrm{C}_1\mathrm{C}_3\log b_{13}+\mathrm{C}_2\mathrm{C}_3\log b_{23}+\ldots),\end{aligned}\right\}\quad(10)$$

with the sole condition

$$C_1 + C_2 + C_3 + \ldots = 0.$$

Let there be only four wires, 1 and 3 for one circuit, 2 and 4 for another; then $C_3 = -C_1$, and $C_4 = -C_2$. Substituting in (10),

$$\frac{2T}{l} = C_1^2 \left(\frac{\mu_1 + \mu_3}{2} + 2\mu_0 \log \frac{b_{13}^2}{a_1 a_3} \right) + C_2^2 \left(\frac{\mu_2 + \mu_4}{2} + 2\mu_0 \log \frac{b_{24}^2}{a_2 a_4} \right) + 2C_1 C_2 \times 2\mu_0 \log \frac{b_{14} b_{23}}{b_{12} b_{34}}. \quad . \quad . \quad . \quad . \quad . \quad (11)$$

The coefficient of C_1^2 in (11) is the coefficient of self-induction per unit of length of the circuit conveying the current C_1. Similarly for C_2^2; and the coefficient of $2C_1C_2$ is the coefficient of mutual induction per unit of length of the two circuits.

From (10) we may find the coefficients of induction of suspended wires, the circuit being completed through the earth. Let M be the coefficient of mutual induction, and L_1, L_2 the coefficients of self-induction of two wires of radii a_1, a_2, heights above ground h_1, h_2, horizontal distance apart d, and specific magnetic capacities μ_1, μ_2; then

$$\left.\begin{aligned} \frac{L_1}{l} &= \frac{\mu_1}{2} + 2 \log \frac{2h_1}{a_1}, \\ \frac{L_2}{l} &= \frac{\mu_2}{2} + 2 \log \frac{2h_2}{a_2}, \\ \frac{M}{l} &= \log \frac{d^2 + (h_1 + h_2)^2}{d^2 + (h_1 - h_2)^2}, \end{aligned}\right\} \quad . \quad . \quad . \quad . \quad (12)$$

where μ_0 is made equal to unity.

As a practical case, let

$$h_1 = h_2 = 3 \text{ metres},$$

$a_1 = a_2 = \cdot 002$ metre, $\mu_1 = \mu_2 = 1 + 4\pi\kappa = 315$, if $\kappa = 25$, and $d = \cdot 5$ metre. Then

$$L_1 = L_2 = 173,$$
$$M = 5$$

approximately. Also, if the resistance is 13 ohms per mile, the resistance per centimetre is 80778 c. g. s.; therefore

$$\frac{L_1}{R_1} = \frac{173}{80778} = \cdot 00214 \text{ second}. \quad . \quad . \quad . \quad . \quad (13)$$

6. This time interval being in general very small compared

with $\frac{L}{R}$ for the electromagnet, we may neglect it; and then (5) becomes

$$\frac{R}{R_1}=\frac{1}{\sqrt{1+m^2\frac{L^2}{R^2}}},$$

or $R_1=Lm$ approximately. The resistance of the electromagnet for high speeds varies inversely as the speed to obtain the maximum strength of signals.

Using the value of $\frac{L}{R}$ given in (9), we have

$$\frac{R}{R_1}=\frac{T}{14{\cdot}64\frac{x-y}{x+y}.z^2}.$$

where $T=\frac{2\pi}{m}$.

At 100 words per minute Morse code, T=about $\frac{1}{40}$ second; therefore at this speed

$$\frac{R_1}{R}=585{\cdot}6\frac{x-y}{x+y}z^2.$$

Suppose $x=2$, $y=z=1$ centimetre; then

$$\frac{R_1}{R}=195{\cdot}2;$$

or the resistance of the electromagnet is $\frac{1}{195}$th part of the external resistance to obtain the maximum magnetizing force—a very low result. If $\frac{L_1}{R_1}$ is taken into account, this becomes $\frac{1}{174}$th.

7. Having made the magnetizing force a maximum for a given speed and size of electromagnet, by varying the thickness of the wire, we may next find the ratio between the outer and inner radius of the coil to make the attractive force between the core and a soft-iron armature a maximum. We have

$$\Gamma=\frac{E}{\sqrt{(R+R_1)^2+L^2m^2}}$$

(neglecting L_1), where

$$L=\tfrac{2}{3}\pi^2ln^4(x-y)^2(x^2+2xy+3y^2+24\pi\kappa z^2),$$
$$R=\pi\rho ln^4(x^2-y^2).$$

Also

$$F = \Gamma G,$$

where F is the magnetizing force, and

$$G = 4\pi n^2 (x - y).$$

To make F a maximum, n being variable, we found

$$R^2 + L^2 m^2 = R_1^2.$$

Therefore

$$F = \frac{EG}{R_1 \sqrt{2\left(1 + \frac{R}{R_1}\right)}}.$$

Substituting

$$4\sqrt{\frac{\pi(x-y)R}{\rho l(x+y)}} \text{ for } G,$$

we have

$$F^2 = \frac{\frac{8\pi(x-y)E^2}{\rho l(x+y)}}{R_1\left(1 + \frac{R_1}{R}\right)}.$$

Now

$$\frac{R_1}{R} = \sqrt{1 + \frac{L^2 m^2}{R^2}} = \frac{Lm}{R} \text{ approximately,}$$

$$= \frac{2m\pi}{3\rho} \frac{x-y}{x+y} (x^2 + 2xy + 3y^2 + 24\pi\kappa z^2).$$

Therefore

$$F^2 = \frac{\frac{12E^2}{R_1}}{lm(x^2 + 2xy + 3y^2 + 24\pi\kappa z^2) + \frac{3\rho l}{2\pi} \frac{x+y}{x-y}}.$$

Now the magnetization of the core is proportional to the magnetizing force ; and the attractive force between the core and a soft-iron armature placed close to it is proportional to the square of the magnetization and to the cross section of the core. Therefore, if A is the attractive force,

$$A \propto \frac{z^2}{lm(x^2 + 2xy + 3y^2 + 24\pi\kappa z^2) + \frac{3\rho l}{2\pi} \frac{x+y}{x-y}}. \quad (14)$$

This increases with z; therefore let $z=y$, the inner radius of the coil; let x be constant and y variable; then A is a maximum when

$$\frac{x^2}{y^2}+\frac{2x}{y}+\frac{3\rho}{2\pi y^2 m}\frac{x+y}{x-y}$$

is a minimum; and that is when

$$\frac{2m\pi}{3\rho}x^2=\frac{\frac{y}{x}-\left(1-\frac{y^2}{x^2}\right)}{\left(1-\frac{y}{x}\right)\left(1-\frac{y^2}{x^2}\right)}.$$

The least value of $\frac{y}{x}$ is

$$\frac{y}{x}=\frac{\sqrt{5}-1}{2}=\cdot 618.$$

Using the former value of ρ, viz. $1700\times\frac{4}{\pi}$, also $m=80\pi$ and $x=2$ centimetres, then

$$\frac{y}{x}=\cdot 7 \text{ nearly}.$$

$\frac{y}{x}$ increases very slowly as x and m increase.

In this determination of $\frac{y}{x}$, the outer radius has been supposed to be constant, and the inner variable, and with it the iron core. If, on the other hand, the inner radius is fixed and the outer variable, a different ratio is obtained, viz.

$$\frac{2m\pi}{3\rho}x^2=\frac{\frac{y}{x}}{\left(1-\frac{y}{x}\right)\left(1-\frac{y^2}{x^2}\right)},$$

which gives lower values to $\frac{y}{x}$ than before.

It also appears from (14) that the attractive force varies inversely as the length of the coil. Though the formulæ are only true for long coils, yet it points in the direction of short, flat coils; for the attractive force is also increased by increasing the transverse dimensions of the coil.

8. In paragraph 5 we found $L_1=173$ and $M=5$ approximately for iron wires of a certain size, distance apart, and

height above ground. The ratio

$$\frac{L_1}{M} = 34$$

expresses the ratio of the strength of the sinal wave currents in the wire containing the electromotive force $E \sin mt$ to the corresponding induced currents in the parallel wire, or, rather, the minimum value of that ratio for rapid reversals. For if R_1, L_1 belong to the primary circuit, R_2, L_2 to the secondary, then

$$\Gamma_1 = \frac{E\sqrt{R_2^2 + L_2^2 m^2}}{\sqrt{\{R_1R_2 - m^2(L_1L_2 - M^2)\}^2 + m^2(R_1L_2 + R_2L_1)^2}},$$

and

$$\frac{\Gamma_2}{\Gamma_1} = \frac{Mm}{\sqrt{R_2^2 + L_2^2 m^2}}.$$

As m increases, this approximates to

$$\frac{\Gamma_2}{\Gamma_1} = \frac{M}{L_2},$$

equal to about $\frac{1}{34}$ if there are no electromagnets in the secondary circuit, otherwise much less. It is here assumed that $\kappa = 25$ for the iron wire.

XXV. *On the Effect of Variation of Pressure on the Length of Disruptive Discharge in Air.* *By* J. E. H. Gordon, *B.A., Assistant General Secretary of the British Association**.

[Plate III.]

History.

IN 1834 Mr. Snow Harris stated† that, other things being equal, the length of the spark which an electric machine or Leyden jar will give in air varies in the simple inverse ratio of the pressure. He, however, gives no tables or figures in support of his law.

Sir William Thomson‡ has determined, by means of an absolute electrometer, the difference of potentials corresponding

* Communicated by the Author, having been read before Section A of the British Association, Dublin, 1878.

† Philosophical Transactions.

‡ Proc. Roy. Soc. 1860, Papers on Electrostatics, p. 247.

to the lengths of sparks between flat plates. The extreme length of spark used in his experiments was 1·52 millim.

In the experiments made by M. Masson*, the spark passed either between two balls in the air, or between two similar balls inside a globe in which a more or less complete vacuum could be produced. The distances between the balls could be varied, as well as the pressures. Within the limits of his experiments, he found that the length of spark was inversely proportional to the pressure. The greatest length of spark which he used was 11·1 millims.

In 1843 M. Knochenhauer† worked with a constant length of spark of about $\frac{3}{4}$ inch‡, and measured the electric density required to produce a spark in air at various pressures. Within the limits of his experiments he found that the ratio of the electric density required to produce a spark, to the pressure of the air, increases sensibly as the pressure diminishes. Now it is a simple deduction from Harris's law that the length of spark is proportional to the electric density; and therefore Knochenhauer's results show that the law given by Harris and Masson does not hold for all distances and pressures.

Wiedemann and Ruhemann§ found a purely empirical formula for variations in the lengths of sparks where the longest spark was 9·95 millims.

In the experiments described in this paper, an attempt has been made to determine the ratio of the spark-length to the pressure for distances ranging from 6 inches to 30 inches by means of one and the same apparatus. The experiments also differ from any former experiments with which the author is acquainted, in the fact that an induction-coil was used as the source of electricity instead of an electric machine.

Description of the Apparatus‖ used.

The Coil.—This was a particularly fine instrument, giving a spark of 17 inches (42·5 centims.) in air at the ordinary pressure. It was worked by 10 quart cells of Grove's battery arranged in series. It was provided with a vibrator and with a clock contact-breaker, either of which could be used.

The Air-pump was of the ordinary Tait's construction, but

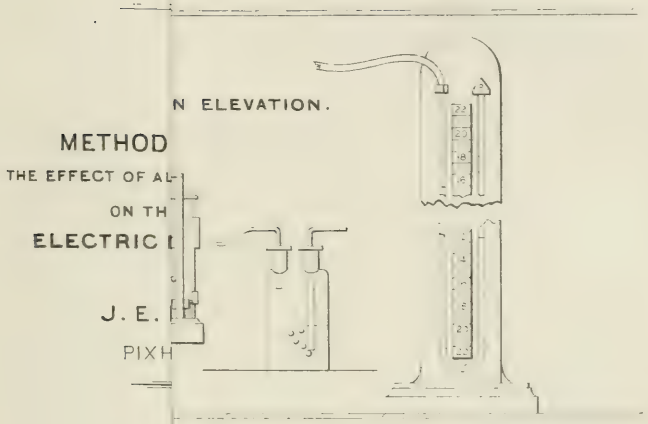
* *Annales de Chimie*, 3e série, t. xxx.; or Mascart, *Electricité Statique*, t. ii. p. 94.

† Pogg. *Ann.* lviii. p. 219; or Mascart, t. ii. p. 95.

‡ He does not state the length of spark he used, but gives the height of his whole apparatus, and a drawing which, if it is to scale, shows that the discharging balls were about $\frac{3}{4}$ inch apart.

§ Mascart, t. ii. p. 97.

‖ The whole of the apparatus was made by Mr. Apps.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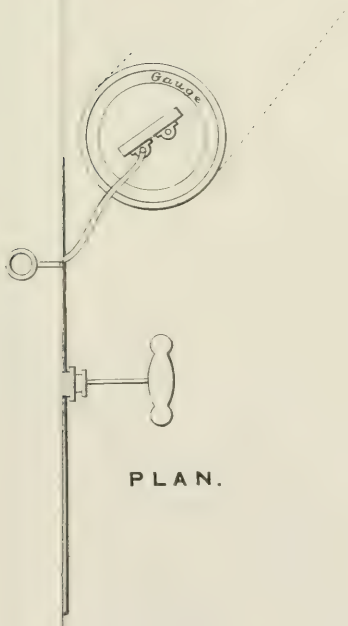

Gauge
PLAN.

METHOD OF MEASURING

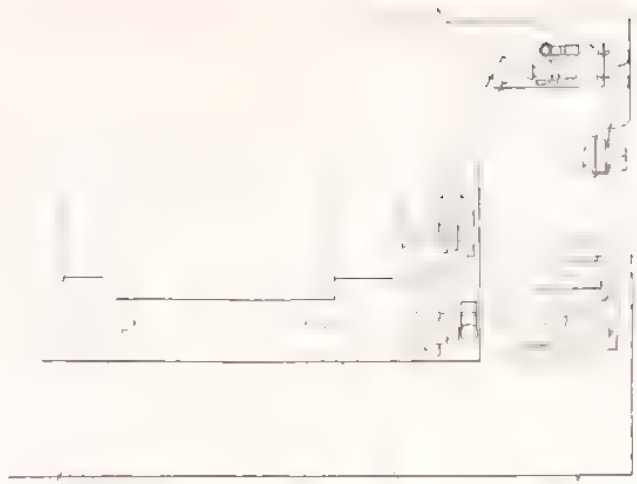
THE EFFECT OF ALTERNATION OF PRESSURE

IN THE ENTHALPY

ELECTRIC DISCHARGES IN AIR

BY

J. E. H. GORDON



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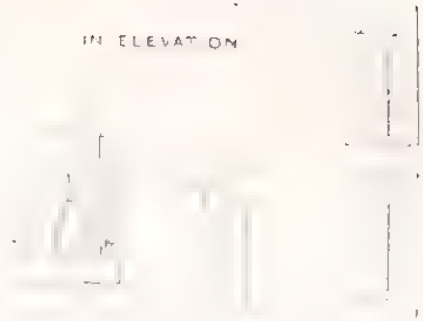
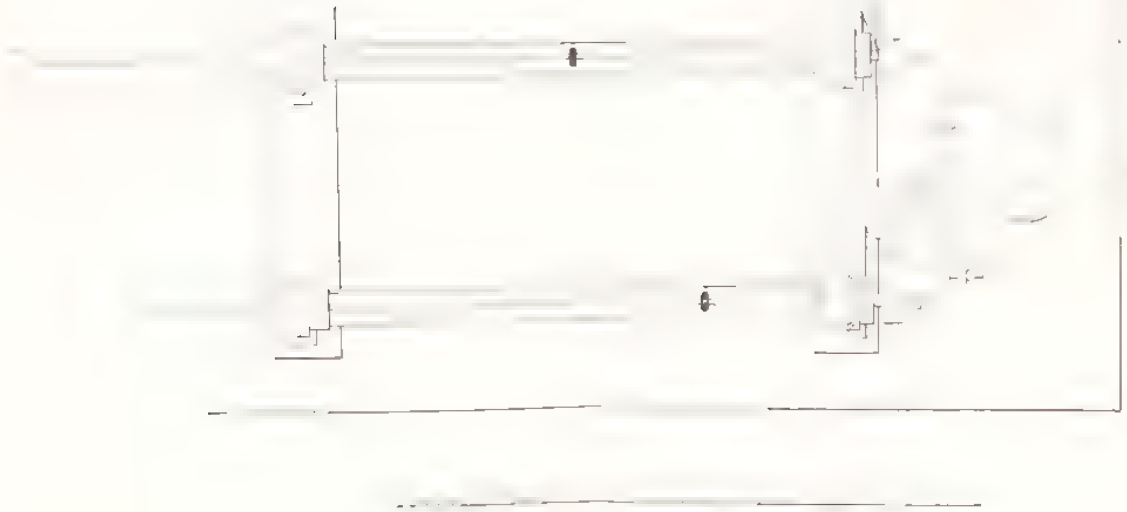




PLAN

was arranged so that more pipes than are usually provided for could be attached to it. Its base being removed, the bolt which had held it to the base passed through a hole in the table ; and the nut being screwed up the pump was firmly fixed.

The Discharging Tubes.—These consisted of two cylindrical glass tubes about 4 feet (1·33 metre) long and nearly 3 inches diameter. At one end of each was a tap, the brass pipe from which ended in a ball which formed one of the discharging terminals. Holes in the side of the brass pipe admitted the air from the tap to the tube. At the other end was a stuffing-box, in which a brass rod slid ; at the end of the brass rod was a point which could either be placed in contact with the ball or withdrawn some 3 feet from it. The end of the rod was kept always in the axis of the tube by means of three little glass arms, which were stuck into an ebonite collar fixed on the discharging rod a little behind the point. The two tubes were supported in a horizontal position, parallel to each other and about 18 inches apart, on four ebonite legs about 18 inches high. The tubes were joined to the air-pump by means of the pipes and taps shown in the figure (Plate III.), which were so arranged that the tubes could be at once connected to each other, to the external air, to a gas-holder, or to the pump. Between the tubes and the pump the metal pipe was cut, and a piece of glass tubing about 18 inches long, well varnished with shellac, was inserted, so that the electricity might not pass to earth through the pump*.

When the tubes were shut off from the pump, air could always be let into the glass pipe to prevent the discharge passing to earth inside it, as it would do at low pressures. The distance between the point and ball in each tube was measured as follows. They were placed in contact, and an ink mark was made on the discharging rod just outside the collar of the stuffing-box. When the rod was slid out, the distance of this mark from the collar was equal to the distance between the point and ball. The pressure was given by a U-gauge, about 4 feet high, attached to the air-pump at one end, open to the air at the other.

The pressure P was given by the formula

$$P = \{\text{height of barometer}\} - \{\text{difference of level of mercury in the two arms of the U}\}.$$

Before being admitted into the tubes, the air was dried by being drawn through sulphuric acid. When it was desired

* Mr. Apps informs me that it is injurious to the coil to connect either secondary terminal to earth when using long sparks.

that the pressure of the air in the tube should equal that of the external atmosphere, air bubbled through the acid as long as the difference of pressure inside and outside the tube exceeded that of the inch of acid which had to be displaced, and then the tap was opened direct to the outside air. The *external* diameters of the tubes were about 2·94 and 2·76 inches respectively, and the diameters of the balls ·94 and ·92 inch.

The Experiments.

In the experiments which are the subject of the present paper, one of the tubes (A) was left open to the atmosphere, and its discharging point placed at a standard distance either 6, 8, or 10 inches from the ball; and the other tube (B) being nearly exhausted, experiments were commenced at the low pressure, and then a little air was let in between each observation. The tubes were so connected to the coil that the discharge would pass in whichever tube offered least resistance. The discharging-distance in B was then varied and adjusted to the shortest distance, which caused the whole discharge to pass in A. The distance between the points of B being noted, the points were then brought nearer together till they reached the longest distance at which the whole discharge passed in B. The mean of these two distances was taken as the distance which, at the pressure then being worked with, interposed in B a resistance equal to that of the standard length in A of air at the pressure of the atmosphere.

Let us call this mean "mean B spark." Now, if the law that the spark is inversely proportional to the pressure holds, we should have for the same series of experiments,

$$\{\text{mean B spark}\}\{\text{pressure in B}\} = \text{const.};$$

and to compare different sets made with different distances in A and with the barometer at different heights, we should have

$$\frac{\{\text{mean B spark}\}\{\text{pressure in B}\}}{\{\text{distance in A}\}\{\text{height of barometer}\}} = \text{const.}$$

If the two tubes and the discharging points were precisely alike, this constant would be unity. Any slight difference in the shape of the points and balls, however, would cause it to differ from unity, but would not affect its constancy.

The Table (pp. 190, 191), which explains itself, gives the results of several sets of experiments arranged in ascending order of pressures.

The results which I deduce from it are:—

(1) From a pressure of about 11 inches up to that of the

atmosphere Harris's law approximately holds good. No variation from it indicating any other law is observed.

(2) No law can be said to be more than approximately true; for when the density has almost reached the discharging limit, any slight accidental circumstance, such as the presence of a grain of dust, a little burning of the point by the last discharge, &c., will cause the discharge to take place. Professor Clerk Maxwell has compared the experiment to the splitting of a piece of wood by a wedge. It is possible to determine the average pressure on the wedge which will split the wood: but in any particular experiment it is impossible to say that the wood will split exactly at that pressure.

(3) When the pressure is diminished below 11 inches, the product in column VII. rapidly diminishes. This shows that at low pressures the spark produced by a given electromotive force is much shorter than is required by Harris's law, or that the electromotive force required to produce a spark of given length is at low pressures greater than that required by Harris's law. This agrees with what Mr. De La Rue has told me, namely that he finds that at all pressures, however low, the discharge is disruptive, and none of it passes by conduction. If any portion could at low pressures pass by conduction, we might expect that a smaller and not a greater electromotive force would be required than that calculated by Harris's law from experiments at high pressures.

It is also not inconsistent with the result of Sir William Thomson's historical experiments (mentioned above) "On the Electromotive Force required to produce a Spark." For he writes*, "Greater electromotive force per unit length of air is required to produce a spark *at short distances than at long*." For the words in italics I substitute "at low pressures than at high." We may then both write "with a low air resistance than with a high one," or "with few air particles between the points than with many." Sir William Thomson says of his result, "it is difficult even to conjecture an explanation;" I can only say the same of mine.

I cannot say exactly at what pressure M. Knochenhauer's experiments show a change in the law—as he evidently considers the change to be due partly at least to experimental errors, and introduces corrections, some of which are apparently suggested by peculiarities in his apparatus, while others are intended to adjust the experimental results to the supposed law. As far as I can see, the change was first beyond the reach of his corrections when the barometer fell to about 2 inches.

* Papers on Electrostatics and Magnetism, § 323. p. 248.

I. Pressure in B = height of barometer minus difference of heights of columns of mercury in the U-gauge.	II. | III. Distance apart of points in B which caused the discharge to take place wholly in A.	B.	IV. Mean of columns II. and III. equal to length of spark which is, at pressure of column I., produced by the same electromotive force which produces a spark in air of length given in column V. when barometer stands at height given in column VI. "Mean B spark."	V. Distance apart of points in A.	VI. Barometer = pressure in A.	VII. Length of spark in B × pressure in B / Length of spark in A × pressure in A = (column IV.) × (column I.) / (column V.) × (column VI.)
inches	inches	inches	inches	inches	inches	
2·42	30·6	22·9	26·75	6	29·64	·364
3·2	36·1	25·5	30·8	10	29·70	·332
4·59	22·7	18·6	20·65	6	29·64	·533
7·8	26·8	21·4	23·9	10	29·70	·627
7·82	22·0	16·6	19·3	8	29·65	·636
9·34	21·1	19·45	20·62	8	29·65	·812
9·49	17·7	16·2	16·95	6	29·64	·904
11·68	19·0	16·8	17·9	8	29·65	·881
11·74	17·2	16·0	16·6	6	29·64	1·095
11·9	13·7	11·8	12·75	6	29·60	·854
11·9	23·8	19·4	21·6	10	29·70	·928
13·5	15·2	13·9	14·55	8	29·50	·821
14·24	14·2	13·3	13·75	6	29·64	1·101
14·29	18·8	15·8	16·3	8	29·65	·982
14·4	23·2	18·8	21·0	10	29·70	1·020
15·1	15·2	13·9	14·55	8	29·50	·931
15·1	12·4	11·2	11·8	6	29·60	1·003
16·05	12·6	12·3	12·45	6	29·64	1·065
16·24	14·9	14·0	14·45	8	29·65	·989
16·3	19·0	16·0	17·5	10	29·70	·960
16·7	15·2	13·3	11·3	8	29·50	·799

17·6	19·1	17·2	18·1	10	29·70	1·072
17·64	11·7	11·1	11·4	6	29·64	1·131
18·2	12·1	10·7	11·4	6	29·60	1·168
18·4	14·8	12·7	13·75	8	29·50	1·072
18·75	13·0	11·6	12·3	8	29·65	·972
19·49	11·3	9·2	10·25	6	29·64	1·123
19·6	16·7	15·3	16·0	10	29·70	1·055
19·8	13·6	11·6	12·6	8	29·50	1·057
20·7	10·6	9·4	10·0	6	29·60	1·165
21·3	13·3	11·2	12·25	8	29·50	1·105
21·54	9·2	8·2	8·7	6	29·64	1·053
21·9	16·8	14·7	15·7	10	29·70	1·157
22·2	15·8	14·1	14·9	10	29·70	1·114
22·54	8·2	7·7	7·95	6	29·64	1·008
22·6	13·0	10·6	11·8	8	29·50	1·130
23·6	15·8	13·7	14·7	10	29·70	1·168
23·7	9·6	8·2	8·9	6	29·60	1·187
24·1	13·1	11·1	12·8	8	29·50	1·307
24·14	7·8	6·9	7·35	6	29·64	·997
24·6	15·2	13·9	14·5	10	29·70	1·201
24·8	8·4	7·0	7·7	6	29·60	1·075
25·1	13·1	10·0	11·55	8	29·50	1·228
25·4	14·8	13·5	14·1	10	29·70	1·205
25·49	7·5	6·7	7·1	6	29·64	1·018
26·2	11·6	9·0	10·3	8	29·50	1·143
26·4	14·3	13·2	13·7	10	29·70	1·218
26·99	6·6	6·2	6·4	6	29·64	·971
27·4	7·3	6·6	6·95	6	29·60	1·072
27·7	10·7	8·6	9·65	8	29·50	1·133
27·7	14·6	12·1	13·3	10	29·70	1·240
28·54	6·3	5·9	6·1	6	29·64	·978
28·6	13·6	12·3	12·9	10	29·70	1·242
29·64	5·6	5·2	5·4	6	29·64	·900
29·7	12·9	11·4	12·1	10	29·70	1·210

XXVI. *Hydrodynamic Problems in reference to the Theory of Ocean Currents.* By K. Zöppritz*.

THE aim of the following pages is to show what are the motions admitted by an unlimited stratum of liquid under external influences acting only upon the surface, supposing that friction takes place in the liquid (as it does in water and all other known liquids to a greater or a less amount).

The differential equations for the motion of frictional liquids are as follows:—

$$\frac{du}{dt}+\frac{1}{\mu}\frac{\partial p}{\partial x}-\mathrm{X}-\frac{k}{\mu}\Delta u=0,$$

$$\frac{dv}{dt}+\frac{1}{\mu}\frac{\partial p}{\partial y}-\mathrm{Y}-\frac{k}{\mu}\Delta v=0,$$

$$\frac{dw}{dt}+\frac{1}{\mu}\frac{\partial p}{\partial z}-\mathrm{Z}-\frac{k}{\mu}\Delta w=0,$$

$$\frac{\partial u}{\partial x}+\frac{\partial v}{\partial y}+\frac{\partial w}{\partial z}=0;$$

in which u, v, w are the velocities in the directions of the rectangular coordinates x, y, z; X, Y, Z are the components of the external forces; p is the pressure, μ the density, k the coefficient of friction of the liquid, and Δ the symbol for the sum of the three partial differential quotients of the second order according to x, y, and z.

The surface-conditions can be expressed most simply thus:—(1) The particles of the surface of the liquid will always remain in contact with those of the adjacent body; that is, both must have the same velocity-components perpendicular to the surface. Naming these ν and ν_1, then must at any time

$$\nu-\nu_1=0.$$

This condition includes, if the adjacent body is itself liquid or gaseous, the necessity that the pressure normal to the surface be equal on both sides, because otherwise the connexion expressed by the preceding equation would be broken. (2) The difference of velocity of the particles of the liquid against those of the body in contact, parallel to the plane of contact, is proportional to the tangential component T of the internal pressure-forces acting upon the surface, and has the opposite direction. Accordingly, if τ and τ_1 denote the tangential velocities of the

* Translated from a separate impression, communicated by the Author, from the *Annalen der Physik und Chemie*, new series, vol. iii. pp. 582–607.

liquid and the contiguous body, the equation

$$T=\lambda(\tau_1-\tau)$$

will subsist, in which λ is the constant of the external friction, depending only on the nature of the two bodies in contact. If wetting of a part of the surface takes place, we must there put $\tau=\tau_1$, consequently $\lambda=\infty$.

Let the liquid be spread over a solid plane. Of external forces gravitation only acts perpendicular to this plane. Let its direction be the positive X-direction; then is

$$X=g, \quad Y=Z=0.$$

Let the solid plane be wetted, so that the liquid particles adjacent to it always remain at rest. This requires that for them $u=v=w=0$ always. The body in contact with the other surface will at every point have equal velocity and in the same direction, but in general dependent on the time. If the initial motion of the liquid was parallel to that of the contiguous medium, or was also $=0$, then, in accordance with the second surface-condition, only motions in the same direction can at any time take place. Consequently, if we place the plane of XZ parallel to this direction, v is always $=0$. The differential equations then become

$$\frac{du}{dt}+\frac{1}{\mu}\frac{\partial p}{\partial x}-g-\frac{k}{\mu}\Delta u=0,$$

$$\frac{dw}{dt}+\frac{1}{\mu}\frac{\partial p}{\partial z}-\frac{k}{\mu}\Delta w=0,$$

$$\frac{\partial u}{\partial x}+\frac{\partial w}{\partial z}=0.$$

The conditions for the particles in contact with the solid surface are

$$v-v_1=0, \quad \dot{T}=\lambda(\tau_1-\tau).$$

If the angles made by the normal to the surface with the X- and Z-axes be denoted by (n, x) and (n, z) respectively, the 1st equation gives

$$(u-u_1)\cos(n, x)+(w-w_1)\cos(n, z)=0;$$

while the 2nd splits up into the two following:—

$$T\sin(n, x)=\lambda(u_1-u); \quad T\sin(n, z)=\lambda(w_1-w).$$

These equations are satisfied if we assume the surface to be horizontal—that is, put

$$\cos(n, x)=1, \quad \cos(n, y)=\cos(n, z)=0,$$

so that the stratum preserves the constant depth h, and if we further put $u=u_1=0$ and

$$\frac{1}{\mu}\frac{\partial p}{\partial x}=g,$$

consequently

$$p=\mathrm{N}+\mu g x.$$

N denotes then the pressure for $x=0$, consequently (if the origin of coordinates is transferred to the upper boundary plane) the pressure, taken as constant, exerted by the contiguous medium upon the surface. Hereby becomes

$$\frac{\partial p}{\partial z}=\frac{\partial p}{\partial y}=0.$$

Since moreover the first of the differential equations is identically satisfied, there remains, for the determination of w, only the third,

$$\frac{dw}{dt}=\frac{k}{\mu}\frac{\partial^2 w}{\partial x^2},$$

because the fourth merely expresses that w is independent of z, and thereby causes the second differential quotient according to z to be omitted in the present equation.

In the condition-equation which must subsist for $x=0$, P now signifies the pressure-component exerted, parallel to the Z-axis, upon an element of the surface perpendicular to the X-axis. According to F. Neuman and Kirchhoff's notation,

$$\mathrm{X}_x=\mathrm{Z}_x=-k\left(\frac{\partial w}{\partial x}+\frac{\partial u}{\partial z}\right);$$

therefore here

$$\mathrm{T}=-h\frac{\partial w}{dx}=\lambda(w_1-w).$$

In the most general case, $w_1=\phi(t)$ is a given function of the time.

The application is not limited to cases in which only low velocities occur, the squares of which can be neglected against the first power; for since

$$\frac{dw}{dt}=\frac{\partial w}{\partial t}+\frac{\partial w}{\partial x}u+\frac{\partial w}{\partial y}v+\frac{\partial w}{\partial z}w,$$

but here

$$u=0,\quad v=0,\quad \frac{\partial w}{\partial z}=0,$$

for the present problem

$$\frac{dw}{dt} = \frac{\partial w}{\partial t}$$

cannot be neglected.

The problem now is, consequently, so to define the function w that, within a space bounded by two planes perpendicular to the X-axis and at the distance h from each other, it satisfies the differential equation

$$\frac{\partial w}{\partial t} = \alpha \frac{\partial^2 w}{\partial x^2}, \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

and the equations

$$(\text{for } x=0) \quad -\frac{\partial w}{\partial x} + pw = p\phi(t), \quad . \quad . \quad . \quad . \quad (2)$$

$$(\text{for } x=h) \qquad w=0, \quad . \quad . \quad . \quad . \quad . \quad (3)$$

and, finally, at the time $t=0$ takes a given initial value—thus,

$$\text{for } t=0, \quad w=f(x). \quad . \quad . \quad . \quad . \quad . \quad (4)$$

For simplification, $\frac{k}{\mu}$ is supposed $=\alpha$, and $\frac{\lambda}{k} = p$.

Hereby, however, the problem is reduced to a well-known thermal problem—to the determination of the temperature in a partition, of which one boundary-plane is kept at the temperature 0, while the other radiates freely into a medium of a temperature given as a function of the time. The long-known solution of this problem shall be considered in what follows, in its relation to some important phenomena of ocean-currents.

The simplest case for calculation, but practically the most important, occurs when the velocity w_1 of the medium in contact is independent of the time, and the motion has become a stationary one and is consequently likewise independent of the time. In this case the differential equation becomes

$$\frac{d^2 w}{dx^2} = 0,$$

and is therefore free from the coefficient of friction. Its solution has the form

$$w = a + bx;$$

and the constants are determined by the two conditions for $x=0$ and $x=h$; so that w becomes

$$= w_1 \cdot \frac{p(h-x)}{ph+1}.$$

The velocity in the surface becomes

$$w_0 = w_1 \frac{ph}{ph+1},$$

and, when h is very great, changes into w_1 itself. Accordingly w can also be written

$$w = w_0 \frac{h-x}{h}.$$

This simple equation solves the important question how the velocity is distributed in a very extensive sheet of water of uniform depth, which is in stationary motion under the influence of winds acting constantly on its surface. It shows that, if the bottom stratum is at rest, motions in the direction of the wind take place in the rest of the strata, with velocities that increase proportionally to the elevation $(h-x)$ above the bottom, from 0 to the value w_0 in the surface. The question put by various authors, mostly geographers, To what depth does the influence of the trade-wind reach? is therefore to be answered thus:—So far as the motion of the ocean under that influence may be regarded as stationary, it extends to the bottom of the sea, and even the deeper strata obey it according to the measure given by the above law, provided that no other causes (*e. g.* displacement-currents) put these strata into other motion. In the case of the deeper strata being kept by an extraneous cause in motion in exactly the opposite direction to that of the upper strata, there must exist between them a plane where the velocity is $=0$. If this be considered as the lower boundary plane of the upper bed, and its distance from the upper surface be denoted by h_1, for the motion in the upper mass the following equation holds good:—

$$w = w_0 \frac{h_1-x}{h_1}.$$

The distribution of the velocity in the upper mass is therefore the same as if the lower were a solid mass.

The velocity found for stationary motion is dependent on the friction-coefficient k only through w_0 (a dependence which vanishes when h is very great); it will consequently diminish with the depth according to the same law in a very viscous liquid as in a very mobile one. In stationary motion the influence of friction appears solely in the participation of *all* the strata in the motion which is communicated from without to the upper surface only.

Dependence on the coefficient of friction first comes in with the consideration of periodically variable motions, and gives (to use a favourite expression with many writers) a measure

for the depth of the penetration of the surface-impulse within a fixed time.

The general solution of the differential equation (1), with the accessory conditions (2), (3), (4), was given by Poisson*. It can be derived more elegantly according to the method (probably originated by Dirichlet) which, in Riemann's Lectures on Partial Differential Equations, p. 140, is applied to the somewhat simpler problem when there is no free radiation, but a given temperature at the surface.

The function w may be composed of two functions u and v, which satisfy the general differential equation (1) for w, and fulfil the following conditions:—

For $x=0$.	For $x=h$.	For $t=0$.
$-\frac{\partial u}{\partial x}+pu=0,$	$u=0,$	$u=f(x),$
$-\frac{\partial v}{\partial x}+pv=p\phi(t),$	$v=0,$	$v=0.$

Their sum, $u+v=w$, obeys then the conditions (2), (3), and (4).

If m denotes the infinite number of collectively real roots of the transcendental equation

$$m\cos mh+p\sin mh=0, \quad . \quad . \quad . \quad . \quad . \quad (5)$$

the demands for u are fulfilled by the following expression (which is to be summed over all the roots m):—

$$u=2\sum_0^\infty \frac{m\cos mx+p\sin mx}{hm^2+p(hp+1)}e^{-m^2\alpha t}\int_0^h f(\xi)(m\cos m\xi+p\sin\xi)d\xi. \quad (6)$$

Those for v are fulfilled by

$$v=\phi(t)\left\{\frac{p(h-x)}{ph+1}-2p\sum_0^\infty \frac{m\cos mx+p\sin mx}{m(hm^2+p(hp+1))}\right\}$$
$$+2\alpha p\sum_0^\infty \frac{m(m\cos mx+p\sin mx)}{hm^2+p(hp+1)}\int_0^t \phi(\lambda)e^{-m^2\alpha(t-\lambda)}d\lambda. \quad (7)$$

The first member serves only for representing the function for $x=0$, and vanishes as often as $0<x\leqq h$; for its second term is nothing more than the development of the first according to the sine of the argument $(h-x)$ multiplied by the roots of the transcendental equation (5), and consequently disappears together with the first term whenever $x>0$. For the application to internal points this first member can therefore be

* *Journal de l'École Polytechnique*, cah. xix. p. 69: and *Théorie Mathématique de la Chaleur*, p. 327.

omitted. Accordingly, for such points it is:—

$$v = 2\sum_0^\infty \frac{m\cos mx + p\sin mx}{hm^2 + p(hp+1)}\, e^{-m^2\alpha t}\int_0^h f(\xi)(m\cos m\xi + p\sin m\xi)\,d\xi$$

$$+ 2\alpha p\sum_0^\infty m\,.\,\frac{m\cos mx + p\sin mx}{hm^2 + p(hp+1)}\int_0^t e^{-m^2\alpha(t-\lambda)}\phi(\lambda)\,d\lambda. \quad . \quad (8)$$

The first of these two members, which proceeds from the initial state, vanishes if $f(x)=0$, consequently if at the time $t=0$ the entire mass was at rest; but it vanishes also with any value of $f(x)$ if t is very great—that is, if the initial state lies in a very remote past. In both cases the motion in the interior of the fluid is represented by the last member alone. The regularities which result from this expression relative to the motion at different depths and at different times are for this problem partly the same as those pointed out by Fourier in the simpler problem when the temperature of the surface is given; for they agree so far, that they depend on the integral according to λ, which is common to both problems.

If $\phi(t)=w_1$ is a given quantity independent of the time, this integral becomes

$$\int_0^t w_1 e^{-m^2\alpha(t-\lambda)}d\lambda = \frac{w_1}{m^2\alpha}(1-e^{-m^2\alpha t}),$$

and the part which is independent of the initial state

$$v = 2pw_1\sum_0^\infty \frac{m\cos mx + p\sin mx}{m\left(hm^2 + p(hp+1)\right)}(1-e^{-m^2\alpha t}). \quad . \quad . \quad (9)$$

From this it is evident, first, that after an indefinitely long time the exponential vanishes, and v in the limiting case is expressed by the sum of which it has been above remarked that it becomes

$$= w_1\frac{p(h-x)}{ph+1} = w_0\frac{h-x}{h}, \quad . \quad . \quad . \quad . \quad . \quad (10)$$

as had resulted from the direct consideration of the stationary state.

If at any time $\theta < t$ a change of the velocity w_1 affecting the surface occurs to the amount of $+\gamma$, then is

$$\phi(\lambda) = w_1 \qquad \text{from } \lambda=0 \text{ to } \lambda=\theta,$$
$$\phi(\lambda) = w_1+\gamma \text{ from } \lambda=\theta \text{ to } \lambda=t;$$

consequently the above integral divides into two and has the value

$$\frac{w_1}{m^2\alpha}(1-e^{-m^2\alpha t}) + \frac{\gamma}{m^2\alpha}(e^{-m^2\alpha\theta} - e^{-m^2\alpha t}).$$

The second member, which accordingly is added to v, repre-

sents the influence of the change of velocity γ propagated in the interior. If t and θ are enormously great, but their difference little, the second member approximates to the value

$$\gamma(t-\theta).$$

The influence of that alteration is then at every depth proportional to its duration and amount.

If the velocity of the contiguous medium is, at the surface, a periodic function of the time—for example,

$$\phi(t)=\cos(at-b),—$$

then, if $t-\lambda=\rho$ be introduced as a new variable, becomes

$$\int_0^t e^{-m^2\alpha(t-\lambda)}\cos(a\lambda-b)d\lambda=\cos(at-b)\int_0^t e^{-m^2\alpha\rho}\cos a\rho\,d\rho + \sin(at-b)\int_0^t e^{-m^2\alpha\rho}\sin a\rho\,d\rho.$$

After an indefinitely long time these two integrals become constant with respect to the time, and consequently the velocity at every depth becomes a periodic function of the time, of the same period $\left(\frac{2\pi}{a}\right)$ as that of the contiguous medium, but of changed amplitude, dependent on x, and with shifted period of occurrence of maxima and minima. If $\phi(t)$ is a periodic function of general character, it can be represented under the form

$$\phi(t)=w_1+w'_1\cos(at-b_1)+w'_2\cos(2at-b_2)+\ldots$$

Putting this value in the integral, we obtain for w an expression, the first member of which changes, for $t=\infty$, into

$$\frac{p(h-x)}{ph+1}w_1,$$

upon which follows a series of members with $\cos(nat-b)$ and $\sin(nat-b)$, of the same sort as in the previous more simple case for $n=1$.

If we wish to calculate the mean velocity during a period $\mathrm{T}=\frac{2\pi}{a}$, all terms affected with sine and cosine vanish from the time-integral, and there remains

$$\frac{a}{2\pi}\int_0^{\mathrm{T}} w dt=w_1\frac{p(h-x)}{ph+1}$$

as the mean velocity, consequently the same as with the stationary motion.

In the problem of the determination of the temperature in the interior of a mass bounded only by one plane, and unlimited in the positive X direction, when the temperature of the plane is given, some other simple laws respecting the increase of temperature with the depth can be deduced, which will be communicated *infrà*. Such laws cannot be obtained for the present problem. If equations (5) to (8) be applied to a stratum of infinite thickness, putting therefore $h=\infty$, for the solution of the transcendental equation

$$mh\cos mh+ph\sin mh=\phi\cos\phi+ph\sin\phi=0$$

it is only necessary that

$$\sin\phi=\sin mh=0,$$

and therefore

$$mh=n\pi,$$

where n denotes a whole number: n then becomes a quantity that increases continuously from 0 to ∞; for the difference between two successive roots m becomes

$$\frac{\pi}{h}=dm.$$

Accordingly we obtain from equation (8),

$$w=u+v=\frac{2}{\pi}\int_0^\infty e^{-m^2\alpha t}\frac{m\cos mx+p\sin mx}{m(m^2+p^2)}\,dm\int_0^\infty f(\xi)(m\cos m\xi+p\sin m\xi)d\xi$$

$$+\frac{2\alpha p}{\pi}\int_0^\infty\frac{m(m\cos mx+p\sin mx)}{m^2+p^2}dm\int_0^t\phi(\lambda)e^{-m^2\alpha(t-\lambda)}d\lambda. \quad (11)$$

Of this, only the second part, v, independent of the initial state, shall be further considered, and, indeed, for the case that $\phi(\lambda)=w_1$ is constant. By inserting after λ the value already used in equation (9) of the integral then resulting, and noticing that the sum (independent of t) takes the value of equation (10) and therefore for $h=\infty$ the value w_1, we get

$$v=w_1-\frac{2pw_1}{\pi}\int_0^\infty\frac{\cos mx}{m^2+p^2}e^{-m^2\alpha t}dm-\frac{2p^2w_1}{\pi}\int_0^\infty\frac{\sin mx}{m(m^2+p^2)}e^{-m^2\alpha t}dm.$$

These two definite integrals can be reduced to Kramp's integral. The reduction of the first is carried out in Riemann's *Vorlesungen über partielle Differentialgleichungen*, pp. 166–169. If for abbreviation we put

$$p\sqrt{\alpha t}=\tau,\quad \frac{x}{2\sqrt{\alpha t}}=\xi,$$

then is

$$2p\int_0^\infty \frac{\cos mx}{m^2+p^2}\, e^{-m^2at}dm = \sqrt{\pi}e^{\tau^2}\left\{e^{2\tau\xi}\int_{\tau+\xi}^\infty e^{-z^2}dz + e^{-2\tau\xi}\int_{\tau-\xi}^\infty e^{-z^2}dz\right\}.$$

The second integral is obtained from this by integrating according to x from 0 to x. The resulting double integrals are reduced by partial integration to single ones; and we get

$$2p^2\int_0^\infty \frac{\sin mx}{m(m^2+p^2)}\, e^{-m^2at}dm$$
$$= \sqrt{\pi}e^{\tau^2}\left\{e^{2\tau\xi}\int_{\tau+\xi}^\infty e^{-z^2}dz - e^{-2\tau\xi}\int_{\tau-z}^\infty e^{-z^2}dz + 2\int_0^\infty d\xi e^{-\tau^2-\xi^2}\right\}.$$

With this becomes

$$w = w_1 - \frac{2w_1}{\sqrt{\pi}}\left\{e^{p^2at+px}\int_{p\sqrt{at}+\frac{x}{2\sqrt{at}}}^\infty e^{-z^2}dz + e^{-p^2at}\int_0^{\frac{x}{2\sqrt{at}}} e^{-z^2}dz\right\}. \quad (12)$$

To obtain from this the more general expression for $w_1 = \phi(\lambda)$, we have only to take the second part with the plus sign, insert $(t-\lambda)$ in the place of t, multiply by $\phi(\lambda)d\lambda$, and integrate from 0 to t.

The velocity in the surface-stratum is obtained from equation (12) by putting therein $x=0$:—

$$w_0 = w_1\left\{1 - \frac{2}{\sqrt{\pi}}\, e^{p^2at}\int_{p\sqrt{at}}^\infty e^{-z^2}dz\right\}.$$

The second part of this expression approximates, for increasing time, to the reciprocal of $p\sqrt{\pi at}$, and therewith to 0 Putting this in words, it means:—The velocity of the surface-stratum continually approaches nearer to that of the contiguous medium.

This necessary inference of the theory, however, draws a limit to its applicability to the theory of ocean-currents. Experience teaches that nowhere do the surface-waters of the sea take the mean velocity of the masses of air blowing over them, because, when the wind is rather high, periodic motions of those strata (waves) arise, on the sides of which the wind acts in quite another manner, namely by pressure on the side-surfaces, so that, with still more augmented difference of velocity between air and water, breaches of connexion take place, discontinuous, turbulent movements. Therefore the surface-condition above laid down can only for very inconsiderable velocities correspond to the reality; for greater velocities the water-surface as a whole cannot fulfil it. In this case it is

better to introduce into the problem the progressive motion of the superficial stratum of the ocean (which, indeed, can everywhere be easily determined by observation) as a given quantity, as a known function of the time (eventually as a constant). Thereby a much simpler problem is obtained, which can be mathematically deduced from the foregoing, if in this we put $p=\infty$. The surface-condition then reads, for $x=0$, $w=\phi(t)$; and the transcendental equation is transformed into $\sin mh=0$, the roots of which are

$$m=\frac{n\pi}{h}.$$

Therewith the general expression of equation (8) for $p=\infty$ changes into the following:—

$$w=\frac{2}{h}\sum_1^\infty e^{-\left(\frac{n\pi}{h}\right)^2\alpha t}\sin\frac{n\pi x}{h}\int_0^h f(\xi)\sin\frac{n\pi\xi}{h}\,d\xi$$
$$+\frac{2\alpha\pi}{h^2}\sum_1^\infty(-1)^{n-1}n\sin\frac{n\pi x}{h}\int_0^t\phi(\lambda)e^{-\left(\frac{n\pi}{h}\right)^2\alpha(t-\lambda)}d\lambda. \quad (13)$$

For the stationary state we get the same formula as before; but now $w_0=w_1$.

In reference to the periodic motion in different depths the same holds which was indicated above for the more general expression. But if the following formula be applied to water of infinite depth, therefore to $h=\infty$, we still get the laws already mentioned. If, namely, in the expression (11) we put $p=\infty$, the second part, above denoted by v, becomes

$$v=\frac{2\alpha}{\pi}\int_0^\infty m\sin mx\,dm\int_0^t\phi(\lambda)e^{-m^2\alpha(t-\lambda)}d\lambda.$$

The integral according to m can be completed; for it is

$$\int_0^\infty m\sin mxe^{-\gamma m^2}dm=-\frac{d}{dx}\int_0^\infty\cos mxe^{-\gamma m^2}dm.$$

But the latter integral is well known. It is, namely,

$$\int_0^\infty\cos mxe^{-\gamma m^2}dm=\frac{1}{2}\sqrt{\frac{\pi}{\gamma}}\,e^{-\frac{x^2}{4\gamma}}.$$

Accordingly

$$\int_0^\infty m\sin mxe^{-\gamma m^2}dm=\frac{x\sqrt{\pi}}{4\gamma^{\frac{3}{2}}}\,e^{-\frac{x^2}{4\gamma}},$$

Consequently

$$v=\frac{x}{2\sqrt{\alpha\pi}}\int_0^t\frac{\phi(\lambda)e^{-\frac{x^2}{4\alpha(t-\lambda)}}}{(t-\lambda)^{\frac{3}{2}}}d\lambda=\frac{2}{\sqrt{\pi}}\int_{\frac{x}{2\sqrt{\alpha t}}}^\infty\phi\left(t-\frac{x^2}{4\alpha z^2}\right)e^{-z^2}dz. \quad (14)$$

Now, if $\phi(t)$ is constantly $=w_0$, it is at once evident that v becomes the same for pairs of values x, t and x', t', for which the lower limit of the integral is the same; therefore

$$\frac{x}{\sqrt{t}} = \frac{x'}{\sqrt{t'}}; \quad \ldots \ldots \quad (15)$$

i. e. the same velocity occurs at different depths which are to one another as the square roots of the times.

The formula that is valid for a constant superficial velocity w_0, namely

$$v = \frac{2w_0}{\sqrt{\pi}} \int_{\frac{x}{2\sqrt{\alpha t}}}^{\infty} e^{-z^2} dz = w_0 \left\{ 1 - \frac{2}{\sqrt{\pi}} \int_0^{\frac{x}{2\sqrt{\alpha t}}} e^{-z^2} dz \right\},$$

can be made use of to calculate, with the help of the Tables that exist for the value of Kramp's integral, the time that elapses before a point in a given depth reaches a certain velocity. The velocity w_0 of the surface first reaches a point after an infinite time. If we take a point at the depth $x=100$ metres, and inquire after what time it will possess the half-velocity of the surface ($v=\frac{1}{2}w_0$), we have to solve the transcendental equation

$$\tfrac{1}{2} = \frac{2}{\sqrt{\pi}} \int_0^{\frac{x}{2}\sqrt{\frac{\mu}{kt}}} e^{-z^2} dz,$$

in which, for α, its value (see p. 195) is put. Herein $x=100$ metres, and instead of k and μ their values for the liquid in question, are to be inserted. Since, according to Encke's Tables in the *Berliner astronomischen Jahrbuch* for 1834, the integral value 0·5027 corresponds to the upper limit 0·48, the time sought, t, is very approximately determined by the equation

$$\frac{x}{2\sqrt{t}} \sqrt{\frac{\mu}{k}} = 0{\cdot}48.$$

The coefficient of friction for water and very dilute salt-solutions is, at ordinary temperature, according to O. E. Meyer*, about 0·013–0·015. Taking 0·0144 and still putting $\mu=1$, from which the density of sea-water differs only in the third place of decimals, we can put for application to the ocean

$$\sqrt{\frac{\mu}{k}} = \frac{1}{0{\cdot}12}.$$

* Pogg. *Ann.* vol. cxiii. p. 400.

k contains as unit the square centimetre; therefore x must also be expressed in centimetres, and be put $=10000$. We thus get

$$\sqrt{t} = \frac{5000}{0{\cdot}48\,.\,0{\cdot}12},$$

which gives the value of t in seconds, and we find

$$t = 7{,}537{,}000{,}000 \text{ seconds} = 239 \text{ years};$$

so that, if the particles of the surface of an ocean of very great (properly infinite) depth, at rest, begins at a point of time $t=0$ to move forward with a constant velocity, half the velocity of the surface will first prevail at 100 metres depth after 239 years.

If we inquire, After how long a time has a tenth part of the surface-velocity penetrated to 100 metres depth? we find it is 41 years. According to the principle expressed in equation (15), at 10 metres depth the same velocities will prevail at the end of 2·39 years and 0·41 year respectively.

These numbers are very appropriate for giving an idea of the slowness with which changes of velocity are propagated downward; for what has just been calculated for the propagation of a commencing surface-movement in a liquid at rest is just as valid for every change of motion in a moving liquid, since the velocity already present and that which has newly entered are algebraically added together. A stationary current diminishing in velocity linearly with the depth will on this account be only extremely little altered by passing changes of velocity affecting its surface (as, for example, by head-winds or storms), except in the strata nearest to the surface. There will much rather prevail at every deeper-situated point a mean velocity but very slightly variable with the time, and determined by the *mean* velocity at the surface. The latter velocity has the direction of the prevailing winds, and falls and rises with them according to a law which cannot be more precisely determined. This fact of the limitation of considerable action of transitory causes to the strata in the vicinity of the surface is an additional justification of the assumption, made to facilitate the calculation, that the ocean is of infinite depth.

If the velocity of the surface is a periodic function of the time, as are all winds dependent on the seasons of the year and hours of the day, it must be possible to express them by a finite series of the form

$$\phi(t) = \rho_0 + \rho_1 \cos\left(\frac{\pi t}{g} - \lambda_1\right) + \rho_2 \cos\left(\frac{2\pi t}{t} - \lambda_2\right) + \ldots.$$

As has been already shown in the preceding more compli-

cated problem, v must then be representable by a series of cosines and sines of the same period. By making use of formula (14) we obtain the following series*:—

$$v=\rho_0+\Sigma\rho_m e^{-x\sqrt{\frac{m\pi\mu}{2gk}}}\cos\left(\frac{m\pi t}{g}-\lambda_m-x\sqrt{\frac{m\pi\mu}{2g'k}}\right),$$

from which we see immediately that the velocity at every depth has the same period $2g$, but that the amplitudes,

$$\theta_m=2\rho_m e^{-x\sqrt{\frac{m\pi\mu}{2gk}}},$$

diminish as the depth increases. In sea-water

$$\sqrt{\frac{\pi\mu}{k}}=14{\cdot}77.$$

If x be expressed in centimetres and $2g$ in seconds, for $x=100$ metres and the period of one year (consequently for $m=1$, $2g=1$ year) the exponent of e becomes about -26; therefore the amplitude of this oscillation already diminishes to a vanishing fraction, with $x=10$ metres to $e^{-2{\cdot}6}=\frac{1}{13{\cdot}5}$.

If the depths diminish in an arithmetical series, the amplitudes of oscillation diminish in a geometrical series, so that in four depths x_1, x_2, x_3, x_4 so situated that $x_4-x_3=x_2-x_1$ the amplitudes θ_1, θ_2, θ_3, θ_4 are in the ratio

$$\theta_4:\theta_3=\theta_2:\theta_1.$$

A maximum and its succeeding minimum of an oscillation of the duration $2g$ are ever simultaneous in depths separated by a distance

$$x_2-x_1=\sqrt{\frac{2\pi gk}{\mu}},$$

which for $2g=1$ year gives

$$x_2-x_1=11{\cdot}9 \text{ metres}.$$

In order to get a numerical representation of the time required by a surface-velocity w_0 commencing at time $t=0$, and remaining constant, to introduce at the bottom of a previously still ocean of the finite depth h the state of velocity opposite to the stationary (when $w=w_0\frac{h-x}{h}$), the above formula (13) must be employed, in which $f(\xi)$ must be put $=0$ and $\phi(\lambda)=w_0$. We then get

$$w=2w_0\frac{k\pi}{\mu h^2}\sum_1^\infty(-1)^{n-1}n\sin\frac{n\pi x}{h}\int_0^t e^{-\left(\frac{n\pi}{h}\right)^2\frac{k}{\mu}(t-\lambda)}d\lambda,$$

* See the expansion in Riemann's *Vorlesungen*, p. 137.

or, after carrying out the integral,

$$w = w_0 \left\{ \frac{x}{h} - \frac{2}{\pi} \sum_1^\infty \frac{(-1)^{n-1}}{n} e^{-\left(\frac{n\pi}{h}\right)^2 \frac{k}{\mu} t} \sin \frac{n\pi x}{h} \right\}.$$

Since

$$\frac{k}{\mu} \pi^2 = 0{\cdot}1421,$$

for a mean sea-depth of 4000 metres, to be expressed in centims., the exponent of e becomes

$$= -\frac{0{\cdot}1421}{400000^2} n^2 t,$$

extremely little as long as t is not enormously great, the series consequently very feebly convergent. If $t = 10000$ years, four more terms must always be calculated in order to obtain an approximation to within one thousandth of accuracy. If the amount of the velocity w_m in the half depth of the ocean, consequently for $x = \frac{h}{2}$, be required, all terms in the series vanish which contain exact multiples, while the remaining sines assume the value ± 1. We then get, for $t = 10{,}000$ years, exactly, within 0·001,

$$w_m = w_0 \left\{ \tfrac{1}{2} - \frac{2}{\pi} (e^{-0{\cdot}280} - \tfrac{1}{3} e^{-2{\cdot}52}) \right\} = 0{\cdot}037\, w_0.$$

Since after an infinite time the velocity $0{\cdot}5\, w_0$ must prevail at this place, it is evident how inconsiderable a portion of the definitive velocity has penetrated to such a depth only after 10,000 years. On the other hand, for $t = 100{,}000$ years w_m becomes

$$= w_0 \left\{ \tfrac{1}{2} - \frac{2}{\pi} e^{-2{\cdot}80} \right\} - 0{\cdot}461\, w_0.$$

Therefore the velocity has, after 100,000 years, already arrived very near to the definitive value; after 200,000 years it differs from it only 0·002.

From the foregoing theoretical considerations two results especially are obtained, which more or less contradict views hitherto accepted:—first, that the stationary motion proceeding from an invariable surface-velocity makes itself perceptible with linearly diminishing velocity right to the bottom in an unlimited sheet of water, while the view has frequently been expressed that the influence of such surface-currents (as, for instance, the impulse generated in the equatorial regions of the ocean by the trade-wind) extends downward to only very limited depths. Secondly, we have found that all periodic or

aperiodic variable changes in the forces acting upon the surface are propagated into the depths with extreme slowness, the periodic with very rapidly diminishing amplitude. From the combination of these two propositions it follows that the motion of the main body of a sheet of water subject to periodically variable surface-forces is determined by the *mean* velocity of the surface, and that the periodic changes are perceptibly only in a proportionally very thin superficial stratum.

It is hence obvious that hitherto the influence of the friction has in one direction been underrated, and overrated in another:—underrated, inasmuch as it has been believed that that influence could not be regarded as penetrating to such depths; overrated, inasmuch as it has been customary to attribute to friction much too considerable an influence in regard to the propagation of the motions of variable currents. Its action has been still more overrated in another direction, of which the following investigation will give some explanation.

Up to the present time the liquid layer was presupposed unlimited in two dimensions; but the case of laterally bounded currents is also capable of treatment*, and therewith the influence of the sides (the shores) upon the flow can be recognized.

Let the liquid have again the depth h, but be bounded by two parallel perpendicular sides with the distance $2b$ between them, which are wetted by the liquid, and at which, therefore, the velocity is $=0$. If the X-axis be situated as before, but the axis of Y placed perpendicular to the two sides, and the point of origin in the centre of the upper-surface line of the cross section, then the velocity w parallel to the sides is given by the differential equation

$$\frac{dw}{dt} = \frac{k}{\mu}\left(\frac{\partial^2 w}{\partial x^2} + \frac{\partial^2 w}{\partial y^2}\right)$$

and the conditions:—

$$\text{For } x=0,\ -\frac{\partial w}{dx} + pw = p\phi(t,y); \quad \text{for } x=h,\quad w=0;$$

$$\text{for } y=b,\qquad w=0; \qquad \text{for } y=-b,\quad w=0.$$

For the case of stationary motion (which shall exclusively be further pursued here), we have

$$\frac{\partial^2 w}{\partial x^2} + \frac{\partial^2 w}{\partial y^2} = 0;$$

moreover the velocity of the contiguous medium must be in-

* The corresponding thermal problem Lamé has taught us to handle in the briefest and most elegant form in his *Leçons sur la Théorie Analytique de la Chaleur*, p. 327.

dependent of t, and therefore only $=\phi(y)$. These requirements are satisfied by the function

$$w=\frac{1}{b}\sum_{0}^{\infty}\frac{\mathfrak{Sin}\,(2n+1)\pi\frac{h-x}{2b}\cos(2n+1)\pi\frac{y}{2b}}{\mathfrak{Sin}(2n+1)\pi\frac{h}{2b}+\frac{(2n+1)\pi}{2bp}\mathfrak{Cof}(2n+1)\pi\frac{h}{2b}}$$

$$\int_{-b}^{+b}\phi(\lambda)\cos(2n+1)\pi\frac{\lambda}{2b}\,d\lambda,$$

in which, for brevity, the hyperbolic sine and cosine are represinted by $\mathfrak{Sin}$ and $\mathfrak{Cof}$; so that

$$\mathfrak{Sin}\,\alpha=\tfrac{1}{2}(e^{\alpha}-e^{-\alpha}),\quad \mathfrak{Cof}\,\alpha=\tfrac{1}{2}(e^{\alpha}+e^{-\alpha}).$$

If the velocity of the air be taken as independent of y, and consequently $\phi(y)$ be put $=w_1$, then becomes

$$w=\frac{4w_1}{\pi}\sum_{0}^{\infty}\frac{(-1)}{2n+1}\frac{\mathfrak{Sin}\,(2n+1)\pi\frac{h-x}{2b}\cos(2n+1)\pi\frac{y}{2b}}{\mathfrak{Sin}(2n+1)\pi\frac{h}{2b}+\frac{(2n+1)\pi}{2bp}\mathfrak{Cof}(2n+1)\pi\frac{h}{2b}}.$$

If herein we put the breadth $2b=\infty$, the resulting formula will be the same as that for the velocity of the stationary motion in an entirely unlimited sheet. The fraction under the symbol of summation takes for $2b=\infty$ the form $\frac{0}{0}$. By differentiating both numerator and denominator according to $\frac{1}{2b}$ we get, after inserting $2b=\infty$,

$$w=\frac{4w_1}{\pi}\cdot\frac{p(h-x)}{ph+1}\sum\frac{(-1)^n}{2n+1}\cos(2n+1)\pi\frac{y}{2b}.$$

But this sum is, for all positive values of y which are less than b (the value $y=b$ itself is excluded), equal to $\frac{\pi}{4}$; so that in reality w receives the above-found value (10).

The motion in the surface, consequently for $x=0$, if the assumption be retained that $\phi(y)=w_1$, is represented by

$$w_0=\frac{4w}{\pi}\sum_{0}^{\infty}\frac{(-1)^n}{2n+1}\frac{\cos(2n+1)\pi\frac{y}{2b}}{1+\frac{(2n+1)\pi}{2bp}\mathfrak{Cot}(2n+1)\pi\frac{h}{2b}},$$

which expression, for $p=\infty$, changes into w_1 itself.

If this series were capable of summation, although only for $y=0$, it might be possible by suitable experiments to ascer-

tain, at least approximately, the value of p with low velocities. For if we imagine a very long canal of given rectangular cross section (for the sake of simplicity let it be taken as square, consequently $2b=h$) set in stationary motion by a gentle wind with the velocity w_1 blowing parallel to its axis, the velocity in the middle line of its surface ($y=0$) will be

$$w_{00}=\frac{4w_1}{\pi}\sum_{0}^{\infty}\frac{(-1)^n}{(2n+1)\left\{1+\frac{(2n+1)\pi}{hp}\mathfrak{Cot}\,(2n+1)\pi\right\}}.$$

Remarking that, for $n=0$, $\mathfrak{Cot}\,\pi$ is already $=1{\cdot}0037$, and for higher values of n the value of $\mathfrak{Cot}\,(2n+1)\pi$ continually approaches nearer to unity, we find as a very close approximation:—

$$w_{00}=\frac{4w_1ph}{\pi}\left\{\frac{1}{ph+\pi}-\frac{1}{3}\frac{1}{ph+3\pi}+\frac{1}{5}\frac{1}{ph+5\pi}-\cdots\right\}.$$

If next we consider only two terms of this series, we get for ph the quadratic equation

$$p^2h^2(8w_1-3\pi w_{00})+ph\,.\,4\pi(8w_1-3\pi w_{00})-9\pi^3w_{00}=0.$$

according to which

$$ph=2\pi\left\{\sqrt{1+\frac{9\pi w_{00}}{4(8w_1-3\pi w_{00})}}-1\right\}.$$

Were, for example, $w_1=4m$, $w_{00}=2m$, consequently $w_1:w_{00}=2$, we should obtain $ph=0{\cdot}88\pi$, or

$$p=\frac{2{\cdot}77}{h},$$

and the first neglected term in the above series would be $=\frac{1}{29{\cdot}4\,.\,\pi}$, while the first term is $=\frac{1}{1{\cdot}88\,.\,\pi}$. The error would therefore amount to about $\frac{1}{16}$ of the value, and would be reduced to about $\frac{1}{30}$ by taking a further term into account.

It appears, however, that p between water and air is very great, and on that account the ratio $w_1=2w_{00}$ possible only when the canal is very narrow, while with a greater width w_{00} approaches much nearer in value to w_1. Many more terms of the above series would then have to be taken into account. The greater the value of p the nearer does the value of w_0 come to that of w_1; consequently the curve which represents w_0 as a function of the distance y from the middle approximates to a straight line parallel to the axis of Y. The same can be said of the velocity in any horizontal line at the depth x. It is therefore evident that, in case p is very great, the banks exert a very inconsiderable influence on the distribution of the velocity.

The stationary state here also is independent of the friction-coefficient.

It follows, moreover, from these considerations, that, in a liquid layer of constant depth, two currents parallel to the same straight line, but flowing in opposite directions, can well be adjacent without disturbing one another. Their dividing surface is then a vertical plane parallel to their directions, in which the velocity is 0, and which behaves exactly like a solid bank. As long as the forces brought forth by each of the currents continue unchanged, the motions of both remain stationary, and neither current disturbs the other.

The complete analogy shown by the distribution of velocity in a mass of liquid possessing friction, with the heat-distribution in a solid body, gives a hint to pursue it also in another direction. It has already been shown above, how extraordinarily slowly the velocity present at the surface is propagated into the depths, provided here the velocity 0 has previously prevailed. From this it can be conversely inferred that the after-effect of the initial state vanishes with the same slowness. This is represented by the hitherto neglected first term of the formulæ (8), (11), and (13). If in them we put for $f(\xi)$ a constant or a linear function of ξ, the integral according to ξ can be at once carried out, and the entire term dependent on the initial state becomes in all three formulæ a sum of the same form as that which was obtained for the term dependent on $\phi(\lambda)$ when $\phi(\lambda)$ was put = a constant. Since the exponents of e are the same in both cases, the above calculated numbers are valid for the vanishing of the initial state.

If, for example, about 10,000 years ago (therefore at a period of which we have no historic information), by any cosmic event the equilibrium of the sea was so considerably disturbed that strong currents resulted therefrom, the influence of the then existing motions would certainly not yet have totally disappeared from the present condition of the currents —indeed it would at the present time predominantly determine the motion of the ocean in its greater depths—if the earth were entirely covered with an ocean of the uniform depth of 4000 metres. The interruption of the continuity of the ocean by continents and island-masses of irregular shape will contribute to soften down considerably that after-effect of previous states of motion, not so much by the increased friction on the sea-bed as by the reflex and displacement currents everywhere breaking in. Nevertheless, after the above numerical proof of the slow spread of the influence of locally acting changes of motion penetrating the mass, a caution is necessary not to put aside the difficulty of more accurate calculation, as is

usual, by saying "All these motions are rapidly consumed by friction."

It would be possible to determine by observations whether any after-effects of former motions are still present in the ocean. For this purpose it would only be necessary to institute comparative observations of the currents at the most various depths in the central portions of the great equatorial currents and the region of calms. Yet we could not hope to demonstrate small remnants of former motions with a certainty like that with which this is possible in relation to the after-effect of the former higher temperature of the earth by measurements of subterranean temperatures.

Further, those above-calculated numbers give a hint how remote, at the least, we have to imagine the initial state to have been from the present time, or for how long a period, at the least, we must imagine that the trade-winds have blown in their present extent and strength, to justify the assumption that the present state of motion of the equatorial currents is approximately stationary. For this about 100,000 years are required if we take for a basis a mean sea-depth of 4000 metres and take no account of the damping influence of the continents and islands, which must somewhat lessen that number.

That every initial state, however, finally vanishes, and, from the simple law of distribution laid down, gives place to a stationary one, is evident from the form of the series for w, and has, besides, been shown generally for all temperature-problems by K. von der Mühll*.

Giessen, January 5, 1878.

XXVII. *Motions produced by Dilute Acids on some Amalgam Surfaces.* *By* Robert Sabine†.

In the December (1876) Supplementary Number of the Philosophical Magazine I stated an opinion, supported by experimental evidence, that the motions which Erman, Draper, and others have observed in certain electrolytic liquids when in contact with mercury surfaces, and in the circuit of an electric current, are due to displacements caused by oxidation and deoxidation at different points.

In following up the subject since, I have made further experiments, and have noticed some phenomena which are interesting (although perhaps of no practical value), to which, I believe, attention has not hitherto been directed.

* *Math. Annalen*, ii. p. 648.

† Communicated by the Author.

Exp. 1. A rather rich amalgam of lead was filtered through a paper funnel into a clean shallow dish, so as to produce as bright a surface as the nature of the amalgam would allow. Then a drop of very dilute nitric acid was carefully placed upon it by means of a pipette. The acid-drop did not lie still (as it would do upon a surface of pure mercury), but set itself at once into a jerky motion. It gradually contracted its area and then suddenly spread out again, then gradually contracted again, and again suddenly spread, the pulsations being irregular. That portion of the amalgam surface which was exposed by the contraction of the drop was very bright and smooth, and appeared to be free from floating particles of foreign metal which roughened the surface at other points.

In a repetition of the experiment with weaker acid, the drop was observed to march bodily over the amalgam surface, going from one side to the other, and sometimes returning.

Dilute sulphuric, hydrochloric, oxalic, and acetic acids behaved similarly to nitric acid, but in somewhat different degrees.

Amalgams of tin, antimony, zinc, and copper behaved similarly to lead amalgam, but also in different degrees.

It was found that, in order to produce these motions, the acid must be sufficiently diluted to avoid the perceptible formation of gas. The extent to which the dilution might be carried appeared to depend upon the richness and nature of the amalgam. Weak motions were observed with the following:—

Amalgam of	1 oz. water to
Copper	1 drop nitric acid.
Zinc	2 drops „
Antimony	6 „ „
Tin.....	8 „ „
Lead	15 „ „

Exp. 2. Instead of the foregoing metals, which in very dilute nitric acid appear to be all positive to mercury, amalgams of platinum, gold, and silver were next tried, these metals being all negative to mercury. On placing the acid-drop on the clean newly filtered surface, no motion whatever was observed. When a trace of positive metal, however, was added, slight motions were observed.

Exp. 3. A dish containing lead amalgam having a drop of dilute nitric acid on its surface was placed under a small glass receiver. The motions of the acid-drop were observed to be irregular, the edge darting out first in one direction and then in another. The air in the receiver was changed by the steady

injection of oxygen ; and it was observed that the motions immediately became more rapid and, at the same time, more regular all round. When the supply of oxygen was stopped and atmospheric air substituted, the motions resumed their original irregularity.

Exp. 4. The foregoing experiment was repeated by allowing a drop of acid water on the amalgam surface to fall into motion underneath the receiver, and then changing the atmosphere to one of hydrogen. The motions immediately ceased. On letting in the air again, the motions were resumed. In carbonic acid, nitrogen, and coal-gas the motions were instantly arrested ; and by covering the amalgam surface under the receiver alternately with one of these gases and with either atmospheric air or oxygen, the motions could be stopped and reestablished at pleasure.

Exp. 5. On a surface of lead amalgam a drop of dilute nitric acid was placed, and the tip of a lead wire suspended in the drop. Between the lead wire and the amalgam was inserted a delicate reflecting galvanometer. By means of an adjustable shunt across the galvanometer, the first current was reduced until the index fell nearly to zero. Then the shunt resistance was increased so as to obtain the greatest sensitiveness, whilst retaining the light point upon the scale. In this position, whilst Mr. McEniry observed the motions of the acid-drop, I observed the motions of the galvanometer, each giving a signal. In this way it was found that each spreading of the drop was accompanied by a slight excursion of the light-point. The direction of the permanent deflection showed that the amalgam surface was *positive* to the lead wire. The direction of the excursions indicated a slight diminution of this permanent current, or that at the moment of expansion of the acid-drop, the amalgam surface became slightly less electropositive. In the intervals between the expansions, the tendency was for the needle to move in the other direction. Lead is positive to mercury in dilute nitric acid ; therefore this reduced electropositiveness may indicate an increased proportion of mercury on the covered surface at the instant of spreading.

The conclusion which I draw from these experiments is, that the motions in question are due to a portion of the surface of the amalgam becoming alternately oxidized by the air outside the acid-drop, and deoxidized by electrolysis in the interior of the drop. The reciprocal play of these two actions is, I venture to suggest, as follows:—When the acid-drop is placed upon the surface of the amalgam of a metal specifically lighter than mercury, it finds the surface to consist of mercury in which are floating innumerable particles of the foreign metal.

The latter, in rich amalgams, are distinctly visible to the eye, producing a somewhat roughened surface. Electric currents are generated between the mercury and the particles of foreign metal, through the acid. When the foreign metal is positive to mercury, the latter (which by contact with the air is always more or less oxidized) becomes deoxidized underneath the drop of dilute acid and therefore cleaned. The drop has less affinity for the clean than it had for the oxidized surface. Its adhesion is therefore diminished ; and it draws itself together in consequence, leaving a surrounding ring of deoxidized mercury, in which the eye can detect few or no floating particles of foreign metal ruffling the smoothness of the surface. Oxidation of this exposed clean portion of mercury, however, gradually sets in; and when the mercury up to the boundary line of the drop is sufficiently reoxidized, the acid reasserts its affinity for the oxide, and the drop is enabled to spread out again to its original dimensions. Interior deoxidation then goes on again by means of the small surface couples ; and the play of alternate contraction by reduced adhesion, due to deoxidation, and spreading by affinity for the oxide formed by the outer atmosphere, is kept up so long as positive metal and acid last.

The contraction due to deoxidation by the current would necessarily be slower than the spreading due to chemical affinity between the acid and oxidized surface.

In explanation of the acid-drop travelling bodily over the amalgam surface, I would suggest that if, from any reason, the oxidation were more energetic at one side of the drop than at the other, it must lose equilibrium and go on travelling in the direction of the more energetic action, because the mercury surface on that side of the drop would be ready oxidized, whilst on the opposite it would be continually left deoxidized.

Supposing that the foregoing explanation is the right one, there are two points well worthy of noting. First, it would follow that the electropositive metal in the amalgam is to some extent electrically independent of the mercury—asserts, in fact, a certain electrical integrity. Secondly, it would be necessary that the electromotive force between the foreign metal and mercury (say lead and mercury) when in very dilute acid must be greater, or at least have a greater decomposing action, when the one metal is in a state of minute subdivision and its particles in intimate electric contact with the other metal, than we should be led to expect from the behaviour of the metals in separate masses.

An analogy appears to exist between this behaviour and the augmentation of chemical affinity of the couples constructed

by Dr. Gladstone and Mr. Tribe by the deposition of a negative upon a positive metal, described by them at the British-Association Meeting at Bristol in 1875.

Connected with the same subject, but susceptible probably of a different explanation, is the following experiment:—

On a clean dry surface of rich lead amalgam a drop of strong nitric acid is placed. The acid spreads itself out, the amalgam surface underneath it, in the course of a second or two, assuming a dull leaden-grey colour. Suddenly, with a flash, this colour changes to a whitish tint, which gradually deepens again in colour until, after another second or two, another flash restores the whitish tint. This alternation of colour is sometimes repeated steadily many times in succession during several minutes, until an energetic action at length sets in and gas is evolved.

The change from grey to white is always abrupt; the change from white to grey always gradual.

Instead of employing a vessel with lead amalgam, the flashing was equally well observed with the amalgamated surface of a piece of common sheet-lead. A galvanometer placed in the circuit showed that at the instant of the flash the covered surface became less electropositive.

A quantity of lead amalgam placed in a shallow dish and completely covered with strong nitric acid showed changes of colour as before. At each flash it was observed that the area of the amalgam was suddenly contracted; and it reexpanded during the change of colour from white to grey.

A drop of strong nitric acid placed upon a bright surface of lead attacks it gradually, and produces a dull grey colour which is slightly opalescent.

A drop of strong nitric acid placed upon a bright surface of mercury attacks it energetically, producing white crystals and evolving gas plentifully.

The behaviour of the separate materials suggests an explanation of what may happen in the case of the amalgam, viz. that while the surface is turning to a grey colour, the nitric acid is attacking lead; and when it suddenly flashes white, the acid is attacking mercury. The strange part of the phenomenon is the apparent passivity of the mercury for some time after placing the acid upon the amalgam. The explanation which appears to me to be the most probable is, that the first contact of the acid with the amalgam surface results in the production of a thin stratum of lead-salt, which floods the mercury surface and tends to keep the nitric acid for a time separated from it. The acid in course of a second or so, however, diffusing through this stratum, commences an attack upon the

mercury, which is stopped by the stratum of mercury-salt which is formed, and which enables the lead particles to reassert their electropositiveness and be further operated upon. Then a fresh diffusion ensues and the play is repeated.

XXVIII. *The Origin of the Power which causes the Stellar Radiations.* *By* Jacob Ennis*.

IT is important for scientific men to acquire the habit of regarding all matter as having been diffused equally, or nearly equally, through all space. This is the initial point in our history of creation, the earliest period which we know. From this universal diffusion we can trace, by the operation of well-known forces, the origin of the stars. We can understand how the vast sidereal systems, stellar nebulæ, were formed, and how they must remain stationary in space. We can understand the origin of solar and planetary systems, and how these latter systems are all moving through space with inconceivable velocities. We can learn (as I proved in my paper in the 'Philosophical Magazine' for April 1877) that gravity is the force which imparted their velocities to all the stars and to all stellar systems. And now I am to prove that in this same universal diffusion of all matter we can behold the reservoir, the illimitable reservoir, of that force which radiates the undulations of heat, light, and actinism from all the stars. I will point out also that, in the condensation from that primitive diffusion, we can behold the origin of those modifications of matter which we call the simple elements. In that condensation also we can see the first manifestations of several of the physical forces, such as electricity, magnetism, cohesion, heat, and light. The three primitive forces, of whose origin we know nothing, are repulsion, chemical force, and gravity.

The diffusion of matter in a gaseous form, by the ordinary repulsive force, is a store of heat; and this store is greater or smaller in proportion to the amount of diffusion. It is analogous to the removal of a pound weight from the surface of the earth: the further it is removed the greater will be the amount of heat produced by its fall. It has been found that when our atmospheric gases expand from one volume to two, their temperature is lowered, and just 144° F. of heat are absorbed and rendered latent. Every addition of the original volume by expansion renders latent an additional 144° of heat; that is, so much heat loses its character and form of heat, and it reappears as repulsion or diffusion. When compressed again, that

* Communicated by the Author.

diffusion is annihilated, and the same original amount of heat is reproduced. By a thermometer in the receiver of an air-pump the loss of heat by expansion is seen clearly enough; but in the sinking of an oil-well recently in North-western Pennsylvania it was manifest on a magnificent scale. Instead of striking a reservoir of oil, the auger entered an accumulation of gas (a hydrocarbon); and as this gas expanded on issuing with amazing force from the orifice of the well, its own heat was converted into repulsion, and it absorbed the heat from the surrounding atmosphere and from the ground. The watery vapour in the gas and in the air fell down as snow, and the ground all around was frozen: it was like the freezing of carbonic acid by its own expansion.

From the well-ascertained fact that a gaseous diffusion absorbs 144° F. of heat by every increase of its original volume, which heat is converted into the form of repulsion, and is called latent heat, we are enabled to calculate the amount of latent heat in all the higher strata of our atmosphere. A volume of air rising three and a half miles (more accurately 3·43 miles) becomes two volumes, and contains 144° of latent heat; on rising double that distance (6·86 miles) it becomes 4 volumes; therefore 3 new volumes are added, and it contains 432° of latent heat; at 10·29 miles it becomes 8 volumes, and 7 new volumes are added, and therefore it contains 1008° of latent heat; and so on upward, according to the following Table (p. 218), which (with other columns here omitted) was constructed by Mr. Benjamin V. Marsh, merchant and, like Benjamin Franklin, an amateur of science in Philadelphia. It first appeared in the 'American Journal of Science,' July 1853. His paper on many accounts is valuable; and the subject is further carried out in my own paper on Meteors in the Proceedings of the American Association for the Advancement of Science for 1871.

The appearance of meteors is a proof that our atmosphere extends upwards more than 200 miles, and that its loftier regions are wonderfully charged with latent heat. In my paper on Meteors I gave two instances, one in Europe and the other in America, where meteors travelled more than a thousand miles through the air from 40 to 100 miles high, and continued vividly bright through the whole distance until they passed from view. Last year another passed in the same incandescent manner a thousand continuous miles over the United States. I also gave other instances of vertical descents, when invariably the light of the meteors went out before reaching the ground. They prove that in these cases the very bright light comes, not from the meteor itself, but from the air in its

front, which is compressed and made to give out its abundant latent heat. Had the meteors themselves been so vividly incandescent, their brightness would have continued another twinkling of an eye before they struck the earth. Had friction been the cause of the heat, their light would have become brighter in the dense lower atmosphere, where invariably their light goes out, albeit gravity hastens their velocities. I pointed out in that paper the fallacies in the reasonings of high authorities who thought they had proved that meteoric light is due to friction.

Height, in miles.	Number of volumes corresponding to 1 volume at the surface of the earth.	Number of degrees of latent heat.	Number of grains of air in cylinder 1 mile long and 1 foot in diameter. Weight at surface of the earth = 2342847 grains, = 334·69 pounds avoirdupois.
3·43	2	144	1171424
6·86	4	432	585712
10·29	8	1008	292856
13·72	16	2160	146428
17·15	32	4464	73214
20·58	64	9072	36607
24·01	128	18288	18303
27·44	256	36720	9152
30·87	512	73584	4576
34·30	1024	147312	2288
37·73	2048	294768	1144
41·16	4096	589680	572
44·59	8192	1179504	286
48·02	16384	2359152	143
51·45	32768	4718448	72
54·88	65536	9437040	36
58·31	131072	18874224	18
61·74	262144	37748592	9
65·17	524288	75497328	4
68·60	1048576	150994800	2
102·90	1073741824	154618822512	$\frac{1}{512}$
137·20	1099511627776	158329674399600	$\frac{1}{524288}$
171·50	1125899906842624	162129586585337712	$\frac{1}{536870912}$
205·80	1152921504606846976	166020696663385964400	$\frac{1}{549755813888}$

The point of most intense interest in the Table of Marsh is the wonderfully large amount of latent heat in an almost infinitesimally small amount of the air. Look at the lower line in the Table ; how immense is the physical force ! and how minute is the amount of matter! and yet the two are physically coupled. Here is a wonder which leads to the very gravest consequences. It is an image of the sun. The amount of matter in that great orb is infinitesimally small when com-

pared with his physical force—that force which has sent out the solar radiations many millions of millions of years. Now mark the origin of that solar force. When our sun in its nebulous period was expanded less than halfway to the nearest fixed star (α Centauri), it was 666,000,000,000,000,000 times more rare than hydrogen. It was more rare than the highest strata of our atmosphere; and its repulsive force, latent heat, was in greater proportion than in the last line of the Table of Marsh. That force is indestructible. It cannot be lost. During the condensation of the sun it must be stored up in the solar elements, and pass off as solar radiation. I speak not now of the falling together of the materials of the sun by the force of gravity, and the consequent liberation of heat from that source. That is exceedingly small, not worth counting, although it might continue the solar radiations 20,000,000 years.

But in the repulsive force which so vastly expanded the nebulous sun we behold only one of the sources of its present power. There was still another and a greater power then residing in our nebulous sun. It was a power great enough to overcome the repulsive force, to condense the nebulous sun to his present liquid condition, and to store up all that infinity of latent heat in the chemical elements of the sun. This paramount and overcoming power was the chemical force. When all matter was diffused through all space, its condensation could not have been caused from the loss of its heat by radiation—in common phrase, by "the cooling of the primitive fire mist." If all space had been thus filled by sensible heat ("fire mist)," then that heat must have remained. It could not radiate away; for there was no other space where it could go. Therefore we must look to some other well-known cause for the condensation of the primitive solar gases. The only other cause we can think of is chemical action; and that is an adequate, a normal, and a familiar cause. The chemical force, indeed, is the great condensing-power in the universe. Oxygen and hydrogen, when under its influence, are rapidly condensed nearly 2000 times in volume to form water: but the repulsive force of these gases is not lost; it is converted partly into heat and light, and partly into cohesion.

We will now attend to some well-known facts to illustrate how the chemical force may overcome the repulsive force, and imprison in a dense solid or a liquid vast stores of many kinds of physical forces. Gunpowder, nitroglycerine, dynamite, and mercurial fulminating powders are examples. In all these cases the original repulsive force has been overcome and appropriated by the chemical force. As being most familiar, we will confine our analysis to gunpowder. Its power is stored

up in its chemical force, and in the repulsion of its three gases, oxygen, nitrogen, and carbonic acid. These gases were floating about in the air until the chemical force laid hold on them, and pressed them down into the small solid materials of the powder. The nitrogen and oxygen were quietly combined as nitric acid, and then united with the potassa of the earth, forming nitrate of potassa, or saltpetre. The gaseous carbonic acid was absorbed through the stomata of plants, then dissolved, and its carbon hardened into wood, or rather the charcoal of the wood. This saltpetre and charcoal, with a very little sulphur, are the only components of gunpowder. We know how the least spark liberates these imprisoned gases; and how terribly their native repulsive power shows itself, not only with thundering sound, but with the power of thunder, and with the heat of lightning.

The materials of gunpowder show this power, not only in the thunder-like explosion and the lightning-like heat, but also in vast stores of chemical force. One of the elements of saltpetre, composed of oxygen and nitrogen, is the violent aqua fortis, or nitric acid. Two other gases, hydrogen and chlorine, are combined and condensed by chemical force into a very different material called hydrochloric acid. These two acids when combined show their power by dissolving gold as a lump of sugar is dissolved in a cup of tea. Hydrogen and nitrogen also are combined and condensed by the chemical force into the violent ammoniacal gas. This ammonia combines with the nitric acid, and the two are hardened by the chemical force into a solid, the nitrate of ammonia. All these are striking instances of the way the repulsive force may be overcome and converted into the chemical force, this chemical force being lodged in vast abundance in small amounts of the resulting solids and liquids. How inert and comparatively feeble are the chemical properties of the four gases which form aqua regia (oxygen, hydrogen, nitrogen, and chlorine)! But the force residing in them in the form of repulsion is great beyond expression, beyond measurement. Think of the wonderful mechanical force lately found necessary to reduce them to a liquid condition—still retaining their repulsion and ever ready to burst out into their native gaseous state. As they can be so easily reduced to the solid condition by the chemical force, this is an impressive illustration of the great power of the chemical force in overcoming and appropriating to itself the repulsive force. The great idea here conveyed is that the repulsive force of these gases cannot be annihilated. It completely disappears as repulsion when these gases are solidified or liquified; but it is converted into the chemical force.

Two great ideas are now clearly proved. First, the inconceivably large amount of repulsive force coupled with every single infinitesimally small portion of the sun when in the nebulous condition, as illustrated by the last line of the Table of Marsh. Secondly, the still greater amount of chemical force then residing in the nebulous sun, which overcame the repulsive force and compressed the sun to his present size—as illustrated by that same force when it compresses oxygen and nitrogen into nitric acid, and when it compresses oxygen and hydrogen into water—in the case of nitric acid, retaining a vast store of chemical force, and, in the case of water, producing a large amount of heat and also of cohesion, as I will soon show.

Both these stores of original force, the repulsive and the chemical, must now reside in the compressed sun, the same as great stores of force reside in compressed gunpowder. And no finite mind can pretend to say how long that reservoir of solar force may be able to send out the radiations from the sun. Count up millions of years as we may, we cannot begin to touch the problem. We stand before its immensity bowed in reverence, as when we contemplate the infinity of space and the eternity of time.

In this condensation of the nebulous stars, our sun included, we behold the origin of the so-called simple chemical elements. These are not eternal entities. They are mere modifications of the primitive gaseous diffusion, formed successively as condensation went on. The proofs I cannot epitomize here, as they are already condensedly stated in my volume, 'The Origin of the Stars,' where they occupy nearly the entire Second Part, more than forty pages. In the meteorites there are only about 22 chemical elements, the same monotonous catalogue, more or less complete, coming down at every meteoric fall. In our Earth, after a longer condensation and a more powerful chemical action, the catalogue became extended to 63. In the Sun, after a still longer and more powerful action, the number of simple elements, judging from the fixed lines, must be several hundreds or several thousands. All the stars have different sets of fixed lines, showing that the modifications of matter which we call chemical elements are infinite in number. The same is seen in the planets. Mercury is nine times more dense than Saturn; and therefore the two are composed mainly of different elements. Greater heat might indeed expand the elements of Saturn more than those in Mercury, but not so much as nine times. This is impossible. All solids and liquids fly off into vapour long before even a double expansion. Moreover there is no estimating the millions of years which Saturn had to cool before the origin of Mercury.

In that same Part I collected many facts to show the heterogeneity of matter, and how the peculiar elements of the Sun may possess many thousand times more chemical force than those in our Earth, and thus continue through periods illimitable to burn and to impart solar radiations. The same is true of the facts "dissociation," which have been insisted on so much. The oxide of gold is dissociated with truly an insignificant amount of heat; the oxide of mercury requires only a little more; but the dioxide of carbon requires many tens of thousand times more heat for its dissociation, if indeed such dissociation by heat be at all possible. The peculiar elements of the sun may require we know not how much more. To say that the great heat of the sun must necessarily dissociate its elements, is to say what we do not know, and that in the face of the strongest reasons to the contrary.

I said there are three primitive forces whose origin is unknown —repulsion, chemical force, and gravity. But we perceive that from the operation of these three all the other special forces become manifest, through the great principle of the conversion of force. From the chemical force in ordinary burning come light and heat; from the same force in the galvanic battery come electricity and magnetism. The great force of cohesion is no exception. In the nebulous condition of the sun, at its greatest expansion, we cannot conceive of the operation of the cohesive force; it was the product of the chemical force while overcoming the repulsive force of the nebula. This we see beautifully illustrated in oxygen and hydrogen. By the chemical force they are condensed into water; and Dr. Henry, of the Smithsonian Institution at Washington, first proved that the cohesion between the molecules of water is probably equal to that between the molecules of ice. In raising up a disk 1 inch square from the surface of water, a force equal to the weight of only 53 grains is required; but this is because layer after layer of the water is broken successively, like a strong cord when its many strands are broken one by one (his experiments are described in the fourth volume of the American Philosophical Society, Philadelphia),—whence he comes to the conclusion that the tensile force necessary to break a cubic inch of water, instead of being 53 grains, must be several hundred pounds. The difference between water and ice is not in the amount of cohesion, but in the fixed polarity of the molecules of ice, which by the action of heat are allowed in the water to turn in every direction.

I have now pointed out in a very brief manner some of the many consequences flowing from the primitive diffusion of all matter through all space, and its slow condensation into stars

by the chemical force. A few words more are necessary on the source of solar heat. My paper opposing the mechanical theories of solar heat, the fall of meteorites in the sun, and the fall of the materials of the sun towards its centre, was published in the 'Proceedings,' for 1867, of the Academy of Natural Sciences of Philadelphia; and at its close I expressed a hope that the subject was put at rest for ever. But Mr. Croll has lately brought out an addition to the mechanical theory. He admits the failure of the meteoric theory; and he concedes that the falling together by gravity of the materials of the sun would produce heat enough for only twenty millions of years—an insufficient period. But he supposes that two unluminous stars, each half the mass of our sun, must have been propelled together in opposite directions with velocities of 476 miles per second, and that their collision must have produced heat enough to supply the solar radiations for 50 millions of years. This vast amount of heat would have expanded our sun very widely in a nebulous condition; and by the falling together of this nebulous mass, heat enough would be produced to last 20 millions of years more of solar radiations. These 50 millions of years of heat derived from the collision, and these 20 millions derived from the falling together of the solar materials, make together 70 millions of years of solar light and heat. Many objections oppose all this.

1. If the solar heat for 20 millions of years would be produced by the falling together of the materials of the sun, then precisely the same amount of energy, or heat, would be consumed to expand the sun to its nebulous condition. And this would be subtracted from the heat of collision, 50 millions of years. Instead therefore of 70 millions of years, his period would remain only the original 50 millions from collision.

2. Mr. Croll has not proved, nor attempted to prove, that the heat of fifty millions of years would be sufficient to expand our sun far beyond the furthest planet, so that in condensing the solar system might be formed. When expanded only to the orbit of Neptune the sun was 14,000,000 less dense than hydrogen. By the Table of Marsh this would absorb and render latent a wonderful amount of latent heat, which Mr. Croll must prove could be produced by his theoretical collision.

3. If our Sun were expanded beyond the orbit of Neptune by a sudden production of heat from any cause, then, in order to contract to nearly his present size, that heat must first radiate away. According to this scheme, the very fact of solar condensation presupposes the loss of heat by radiation. But after the loss of all this heat which caused the expansion (the heat of 50 millions of years), none at all can be left for solar

radiation after the formation of the planets. The theory leaves the solar system from its first creation in total darkness and inconceivably cold.

4. Mayer, the author of the meteoric theory, could not account for the former fused condition of our globe, and its present interior heat, by the fall of meteorites. He therefore supposed that two opaque stars had collided to form our Earth in a molten condition. Then he is bound to do the same with the other planets. In each case two planets must have collided, leaving the resulting planet in a nebulous state to form its satellites by condensation. And, *mirabile dictu*, all the collisions must have been so nicely adjusted that the resulting planets should rotate in the same direction on their axes, and revolve in the same plane and in the same direction round the sun, the same direction that the sun rotates, and nearly in the solar equatorial plane. The chances against all this scheme of collisions are infinite; and therefore it is impossible. Mr. Croll's theory is about the same. The sidereal systems are as orderly as our solar system. Our own sidereal system is composed chiefly of the ring of the galaxy, like the ring of the asteroids, and like the rings of Saturn. The ring in Lyra, with other sidereal rings, and other regular forms of sidereal systems, shows the legitimate working of the nebular theory, but not the chance collisions of wandering stars.

5. Mr. Croll has to assume velocities for his colliding stars such as could not have been produced by the force of gravity. This is his own admission. But I have proved that gravity is the force which in the beginning put all the heavens and the earth in motion. No other force can be conceived to cause the stellar velocities; and that force is sufficient*.

6. Mr. Croll's theory is, that originally all the stars were dark and cold, and in the most rapid motion—far beyond the velocities which could be produced by the force of gravity. The collisions, he says, have stopped them and made them shine: "The fixed stars are suns, and they are visible because they have lost their motions." But the fixed stars have not lost their motions. Every astronomer knows that they have their "proper motions," velocities more rapid than those of the planets. 61 Cygni moves nearly 2000, and Arcturus nearly 3000 miles per minute.

7. There are sound reasons for believing that collisions among the stars is an impossibility. I have proved that the

* See 'The Origin of the Stars,' Trübner and Co., 1st London (from the 4th American) edition, and papers therein referred to; also the article on the Physical and Mathematical Principles of the Nebular Theory, in this Journal for April 1877.

fixed stars in our sidereal system are all under each other's influence through gravity. Two stars approaching each other through gravity would at the same time be under the influence of other neighbouring stars, drawing them from the right line towards each other's centre. Therefore they would not collide; but they might approach very near to each other, so as to remain permanently within their powerfully gravitating force, and form a binary system. This is a mode of accounting for the ten thousand double and multiple stars already known, besides the mode of explanation by the nebular theory. Moreover the nebular theory necessarily gives very high velocities to all the stars, but to all in the same direction. If afterwards, by perturbation, they receive contrary directions, still the motions of any two precisely toward one another would be the most improbable of events.

Therefore the mechanical theory of stellar light and heat utterly fails in all its phases. The only true theory for the "ignes æterni" is that of chemical action. This also has been abruptly denied, but only by those entirely unacquainted with the foundations of that theory as explained by myself in 'The Origin of the Stars' and in subsequent papers.

XXIX. *On Acoustic Repulsion.* *By* V. Dvořák*. *With a Note by* Prof. A. M. Mayer.

1. *Acoustic Repulsion of Resonators which are open at one end only.*—In a previous article, "On Acoustic Attraction and Repulsion." I have conclusively proved by theoretic considerations, as well as by experiments, that the average pressure at the node in a column of air vibrating in stationary waves cannot be equal to zero as long as the amplitude of vibration is not infinitely small.

In a resonator open at one end, as, for example, a cylinder, we find a node at the closed end. In the interior of the cylinder near its closed end there exists a greater pressure than on the outer surface of this end which is touched by the outside air, as can easily be shown by means of a sensitive manometer.

To obtain resonance the opening of the cylinder is turned toward the source of the sound; and the cylinder is then repelled by the excess of pressure within. Resonators not having a cylindrical form, but open at one end, are also subject to such repulsion. In my previous communication I have indicated

* From the American Journal of Science and Arts for July 1878. Translated from the *Annalen der Physik und Chemie*, Band iii. No. 3; dated Agram, November 19, 1877.

means for observing the acoustic repulsion of resonators. As the method described there is not very sensitive, I have replaced it by the following. The resonators here employed are usually made of stiff drawing-paper covered with gum-arabic, and have the shape of a cylinder, with a little paper tube, hf, at one end (fig. 1, A). This little tube may also be omitted (as

Fig. 1.

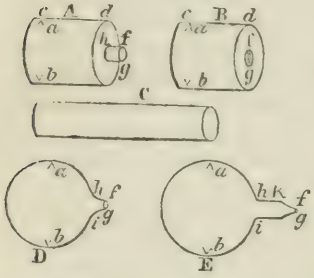
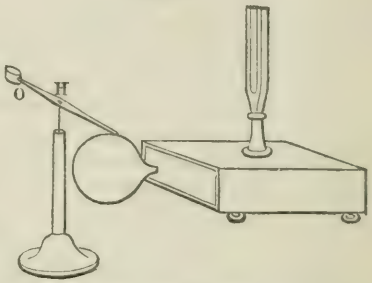


Fig. 2.



in B): in that case the resonator is tuned by increasing or diminishing the little opening, fg. Even a cylindrical tube open at one end, C, may serve our purpose as a resonator. Spherical resonators of glass, D, which a practiced glass-blower can make as light as paper resonators, are excellent. The note of the resonators is determined by gently blowing over the opening or by tapping.

The resonator is fastened with sealing-wax to the end of a light wooden rod, the other extremity of which is provided with a counterpoise of lead, O (fig. 2). The centre of the rod has a glass cap, H, which rests on a needle-point.

The best source of sound is a resonant box of a tuning-fork (fig. 2). The repulsion is so great that it is apparent even with an ordinary brass Helmholtz resonator weighing, with the lead counterpoise, 142 grams*. With every tuning-fork we must first ascertain whether the air in the resonating box vibrates with sufficient energy, because this is not always the case even with accurately tuned boxes. As the elasticity of the different boards which form the elastic system of the box is not equal, their vibrations may hinder the formation of the node at the bottom of the box; in this case the air on the bottom of the box will vibrate but feebly. We can easily ascertain this fact by accurately tuning the box to the note of the fork, and then observing whether the note is considerably weakened by partially covering the opening. If it is not, then the air in the

* The apparatus represented in fig. 3 may also be used to show acoustic attraction by turning the closed end of the resonator toward the box.

box has but little vibration, even if the tone of the fork is powerful. I have, for example, two boxes with excellent tuning-forks by König (of 256 vibrations per second), in which the air would in no wise vibrate powerfully. The strength of the vibration of the air was considerably affected by the degree of tightness with which the fork was screwed to the top of the box. The fork is always vibrated powerfully with a bow; and two bits of rubber tubing must be on the bottom of the box. I generally use the fork A_3, of 435 vibrations per second, by König. Repulsion is then plainly visible with glass resonator at a distance of 10 centimetres from the opening of the box. With a large C fork of König (of 128 vibrations) which sounds for more than ten minutes, it was apparent at a distance of 20 centimetres.

The resonators may be tested either by the reinforcement of the sound produced with a tuning-fork, or by the weakening of the sound on approaching them to the opening of the box*. It is not possible to obtain the repulsion of resonators from the prongs of a tuning-fork alone, as their aerial vibrations are too weak (compare Pogg. clvii. p. 42). I formerly tried in vain to obtain acoustic repulsion from vibrating bodies without the aid of resonance. I suspended small resonators before the end of a glass tube vibrating longitudinally, and provided with a cork to increase the vibrating surface. The open end of the resonator was probably too near the end of the fork, and so produced a lowering of the tone and acoustic attraction instead of repulsion. Attraction is probably present in all cases, and can assert itself only when not counteracted by greater repulsion†. Later I obtained repulsion very easily in a longitudinally vibrating glass tube 127 centims. long and 27 millims. in diameter, on the end of which was a cork 46 millims. in diameter. One of the resonators used was spherical (fig. 1, D), and another cylindrical (C).

I also obtained powerful repulsion with a circular disk 31 centims. in diameter and 2 millims. thick, made by König. The plate was fastened in the centre in a vertical position and made to vibrate in six segments, producing a note of 208 vibrations. The resonator was made of stiff paper of the form of B (fig. 1); $a\,b$ equal 80 millims., $c\,d$ 140 millims., $f\,g$ equal

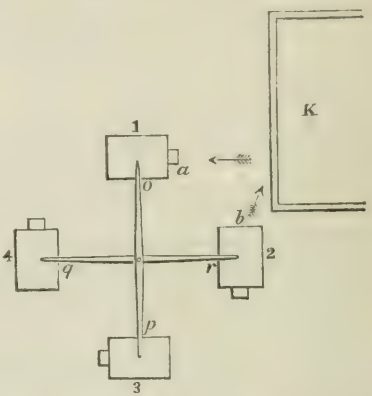
* This is perhaps connected with a conversion of the aerial vibrations in the box into the work of repulsion. The *vis viva* of the sound-vibrations disappears to reappear as work.

† These experiments were also described in a previous communication. In the apparatus represented (fig. 2), repulsion is easily converted into attraction by diminishing the opening of the resonator with wax, and so throwing it out of tune.

17 millims.; and its opening was placed in front of the centre of a vibrating segment, or ventre.

2. *The Acoustic Mill.*—A continuous rotation is easily obtained, on the principle of the acoustic repulsion of resonators, by fastening four very light paper or glass resonators upon two wooden rods, $o\,p$, $q\,r$ (fig. 3), crossing at right angles, and balanced on a glass cap; all the openings of the resonators fronting one side in the direction of tangents. The whole apparatus is placed before the opening, K, of the resonating box and fork, in the manner indicated in fig. 3. The open end a of resonator 1 is repelled from K; the closed end b of resonator 2 is attracted: but in general this attraction does not increase the rapidity of rotation, because it counteracts rotation the moment the resonator (2) has changed its position about 45°. It is therefore not possible to obtain continuous rotation by means of acoustic attraction, as I have shown by numerous experiments*. The resonator (1) continues to move by reason of its inertia, and resonator (2) takes its place, being in turn repelled, and so on.

Fig. 3.



A very rapid rotation is obtained by using a large Kundt's tube and placing a small acoustic mill before its open end.

The glass tube (Kundt's), which vibrates longitudinally and produces the tone, is fastened to a heavy table, and protrudes only a short distance through the cork into the glass tube, placed upon a separate table so that its open end projects somewhat beyond the edge of the latter. The length of the rod was 127 centims., the diameter 27 millims.; the half wave-length of its note, $\frac{\lambda}{2}$, equals $10\frac{1}{2}$ centims. The length of the tube was 45 centims.; the length of the vibrating column of air, corrected for the open end, was $3\frac{\lambda}{2}+\frac{\lambda}{4}$; the inner diameter was 5 centims.

* Instead of the resonators (fig. 3) I used vertical paper vanes, varying e curvature, without achieving any results, notwithstanding the fact that there was a pretty strong acoustic attraction for each separate vane.

3. *The Acoustic Torsion-balance.*—If we hang by a wire a wooden rod provided with a resonator, like the beam of a Coulomb's torsion-balance, in a case having an opening in the side turned toward the resonator, we can compare the intensity of notes having an equal number of vibrations by means of the repulsion of the resonator; but further experiments are necessary to test the practicability of this method. The sound proceeded from an open pipe, having the note A (of 435 vibrations). To prevent the current of air which passes through the pipe from striking the resonator attached to the balance, we must cut the pipe exactly in the middle of its node, and insert a slack membrane softened with glycerine. To prevent the air issuing from the mouth of the pipe from impinging on the resonator, a broad box is used which surrounds the mouth of the pipe air-tight. This box is open on the side opposite the resonator, so as not to impair the tone. The pipe is sounded by means of a König's acoustic bellows with a uniform blast of air. The distance of the resonator from the mouth of the pipe must be at least 2 or 3 centims., to avoid a change of pitch.

4. *Production of Aerial Currents by Sound.*—It may easily be proved by simple theoretic considerations that the mean pressure at the node of a column of air is greater than at its ventre; and that it steadily diminishes in passing from the node to the ventre, provided that the amplitude of vibration is not infinitely small.

It would seem that this difference of pressure would be neutralized by the passage of the air from the node to the ventre. There would then be produced a mean pressure in the whole column, which would be greater, however, than that of air at rest; consequently air would issue from the opening of the vessel in which it forms stationary waves. I have not succeeded, so far, in making the whole process clear; for in reality no perfect balance of pressure takes place. The manometer always shows a slight excess of pressure even at the ventre; but this excess increases as we pass to the node. All my previous experiments indicate moreover that a current of air passes from the node to the ventre, at least in Kundt's tube, in which the air-waves are very powerful. This *principal current* lasts as long as the air vibrates. Besides, the same experiments show a continuous secondary current, close to the walls of the tube and in a direction contrary to that of the principal current; so that the whole air in the tube is in circulation. The cross section of the principal current is nearly as great as that of the tube, while that of the secondary current is a very narrow ring.

The excess of pressure as shown by a manometer at the node is always less than the theoretical pressure, because in the latter the air is not supposed to move from the node and to equalize the pressure. Of course the excess of pressure at the ventre is not equal to zero, as theory requires. Probably the friction of the walls has much to do with these phenomena. It may be expected from what has been said, that the air will issue from the vessel in which it vibrates in stationary waves. The manometer shows, in the first place, that the excess of pressure is not equal to zero in the plane of the opening of a resonator, because a portion of the air immediately in front of this opening partakes of this stationary wave-motion, and because there is always a small excess of pressure even in the ventre of a stationary wave. There is no doubt that a partial equalization of pressure takes place at the opening; experiments show, furthermore, that there is a continuous exit of air, which, as in Kundt's tube, is probably neutralized by a secondary and contrary current.

The exit of the air can easily be proved as follows: a spherical glass resonator is placed before the resonant base of a tuning-fork; the resonator is filled with tobacco-smoke; strong vibrations are given to the fork, when the smoke will be seen to rush from the resonator.

The current of air proceeding from a resonator is well shown by means of a Chladni plate, by means of lycopodium, which accumulates upon the ventres in little heaps when the plate is sounded. If now we place the opening of a bottle (or bottles) of a resonator, D, over such a heap, the lycopodium is immediately blown about in a circle, and may be scattered in any direction by giving suitable inclinations to the resonator. A glass plate held over a heap of lycopodium produces the opposite effect by causing it to contract.

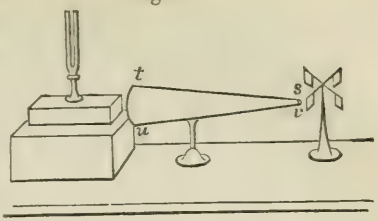
I have succeeded in producing comparatively strong currents of air in still another manner; but I have not yet found an explanation of these complicated phenomena.

A cone made of stiff paper was held with its large end opposite the opening of a large Kundt's tube. The size of this cone may vary; but its effect is greatest when it vibrates to the same note as the Kundt's tube, and so forms a resonator open at both ends; the diameters of its open ends are 37 and 7 millims., and its length 90 millims.

When the Kundt's tube begins to sound loudly, a current of air issues from the narrow end of the cone with such violence that it easily blows out the flame of a candle at a distance of 20 centims. This current rushes through the cone with a peculiar noise, and is easily felt with the finger.

The cone may be replaced by a cylinder having the width of the Kundt's tube, open at the end turned toward the latter, and closed all but a small hole at the end; but the current is much weaker; nevertheless it will move a small wheel with vertical paper vanes (fig. 4).

Fig. 4.



In the experiments with the tuning-forks, it is essential that the cone should vibrate to the same note as the fork; otherwise the current is too weak. For the fork A (of 435 vibrations) the openings of the cone have diameters of 82 and 3 millims., and its length is 373 millims. The opening at the apex of the cone must be very small to obtain an appreciable current.

On conclusion of this investigation, Dr. R. König kindly communicated to me that Mr. Alfred Mayer in New York [Hoboken] had previously succeeded in producing continuous rotation by means of sound. The communication was as follows:—"Professor A. M. Mayer showed me a very similar experiment last summer (1876). He suspended by a thread two large well-tuned flasks attached to a rod, and caused the whole apparatus to revolve by means of a tuning-fork. I informed him in consequence that you had previously demonstrated the phenomena of repulsion in resonators; for he was not acquainted with your paper* on acoustic attraction and repulsion."

Note by Professor Alfred M. Mayer.—My connexion with the discovery of the sound-mill is as follows:—

In January 1876 I made the discovery (first reached by theoretic deductions) that there was more pressure on the inner surface of the bottom of a resounding cavity than on the outer surface of the bottom, which touches the outer air. I subsequently proved the truth of this conclusion by experiments on suspended resonators, and by observations on the motions of precipitated silica powder and films of soap-bubbles placed at various points in resonators of different forms. My first publication of these results was on May 22, 1876, on which day I read a paper on this discovery before the New-York Academy of Sciences, and exhibited before the members an apparatus formed of two + arms of light wood, with a resonator attached to each arm, as in fig. 3 of Professor Dvořák's paper. On sounding an organ-pipe, or a fork on its resonant box, in tune with these resonators, they were successively repelled from the

* Read before the Royal Academy of Sciences, Vienna, in 1875.

pipe, or fork, and a continuous rotation was exhibited. At the same meeting this experiment was preceded by those on the motions of silica-powder &c. in resonators.

On the 8th of July, 1876, there appeared in the 'Scientific American' a report of this meeting of the Academy, in which my experiments on Acoustic Repulsion are thus referred to:—

"In the next place, Professor Mayer exhibited an apparatus constructed by him to produce motion by means of sound-pulses. Four glass resonators on cross-arms were suspended by means of a string. On sounding an organ-pipe in tune with the resonators and bringing it opposite the mouth of one of them, the resonator was repelled and the apparatus commenced to rotate. This experiment was the more striking from the fact that, so far from any current of air proceeding out of the mouth of the organ-pipe, the air is actually sucked in, as may be rendered visible by means of smoke from a cigar. The smoke is carried up the pipe even when the latter is closed at the top with cotton wool so as to smother the sound. On substituting disks of cardboard for the resonators, they were drawn up to the mouth of the organ-pipe with considerable force. When fine silica-powder was placed in the resonators, it was thrown into violent motion on sounding the pipe."

In the same month (July 1876) Dr. Rudolph König visited me, and I exhibited the same experiments before him.

The discovery of the acoustic repulsion of resonators and the invention of the sound-mill were made independently by Professor Dvořák and myself. It is another instance of men, even so far distant as Agram and Hoboken, led into the same path of research by the natural growth of science.

Dimensions of the resonators and reaction-wheels used, in millimetres:—

(1) Fork C, of 128 vibrations. Glass resonator of form E (fig. 1), ab equalled 90; hi, 25; hk, 20; kf, 33; fg, 8. Its weight, together with its leaden counterpoise, was 70 grams.

(2) Fork A, 435 vibrations per second. (*a*) The glass resonator used in the experiment represented in fig. 2, and to show the current of air by means of smoke, was of the form D (fig. 1): ab equalled 58; hf, 22; fg, 10. (*b*) The glass resonators of the acoustic mill were of the form D: ab equalled 34; hf, 12; fg, 3. The length of the arms from the middle of the glass cap to the middle of the resonator was 52 millims. The weight of the whole wheel was 23 grams. (*c*) Paper resonators of the acoustic mill (fig. 3) were of the form A (fig. 1): ab equalled 34; cd, 50; hf, 6; fg, 9. The length of the arms was 65 millims., the weight of the whole wheel 9 grams.

(3) Kundt's tube, $\frac{\lambda}{2}$ equals 105 millims. The glass resonators of the acoustic mill were of the form D (fig. 1): *a b* equalled 24; *h f*, 2; *f g*, 7; length of the arms 30 millims.

It is a striking fact that very small resonators may give a very deep note: with fork A, I used a glass resonator of the form D (fig. 1), in which *a b* equalled 24, *h f* 14, and *f g* 1 millim. The volume was about ninety times less than that of the resonant box of the fork, to whose note the resonator was tuned. Notwithstanding its smallness it showed acoustic repulsion.

XXX. *Proceedings of Learned Societies.*

GEOLOGICAL SOCIETY.

[Continued from p. 153.]

June 5, 1878.—John Evans, Esq., D.C.L., F.R.S., Vice-President, in the Chair.

THE following communications were read:—

1. "On the Quartzites of Shropshire." By Charles Callaway, Esq., M.A., B.Sc., F.G.S.

In a former paper (Q. J. G. S. xxxiii. p. 652) the author indicated that part of the so-called quartzites of the Wrekin are "Hollybush Sandstone;" in the present communication he shows that the whole, both in the Wrekin and Church-Stretton areas, are of Cambrian or Precambrian and not of Caradoc age.

In the Wrekin area the quartzites rest unconformably against the volcanic axis in a nearly continuous band, dipping away from it at angles of from 30° to 55°, their present position being due to its elevation. The volcanic rock is a bedded Precambrian tuff, which reappears in Lawrence Hill and the Ercal, also accompanied by quartzites overlain by Hollybush Sandstone. Caer Caradoc belongs to the same volcanic series; and the quartzites reappear on its S.E. flank, overlain by Hollybush Sandstone containing *Kutorgina cingulata* and *Serpulites fistula*, above which follow the Shineton Shales, and next, separated by a fault, the Hoar-Edge grits (Lower Caradoc). The author believes that the apparently conformable succession here is due to parallel faults. Along the S.E. flank of the Wrekin the quartz rock dips S.E., while the volcanic rocks dip N., and fragments of the latter are contained in its base. The author is inclined to consider this a friction-breccia, and the junction a faulted one. He also regards the junction with the Hollybush Sandstones as a faulted one, and maintains that in any case the quartzites are older than the latter rocks, which are sometimes considered the equivalents of the Ffestiniog group, and by Mr. Belt to be Menevian. The quartzites can hardly belong to any part of the Upper Cambrian; and the author passes on to consider the various positions which they may be held to occupy, and gives reasons for thinking that they are Pre-

cambrian. The only fossil that has been found in them is a supposed worm-burrow. In conclusion the author expresses the opinion that the Stiper-stones quartzites are of Arenig age.

2. "On the Affinities of the Mosasauridæ, Gervais, as exemplified in the bony structure of the fore fin." By Prof. Owen, C.B., F.R.S., F.G.S., &c.

3. "On new Species of *Procolophon* from the Cape Colony, preserved in Dr. Grierson's Museum, Thornhill, Dumfriesshire; with some Remarks on the Affinities of the Genus." By Harry Govier Seeley, Esq., F.L.S., F.G.S., &c., Professor of Geography in King's College, London.

4. "On the Microscopic Structure of the Stromatoporidæ, and on Palæozoic Fossils mineralized with Silicates, in illustration of Eozoon." By Principal Dawson, LL.D., F.R.S., F.G.S.

5. "On some Devonian Stromatoporidæ." By A. Champernowne, Esq., F.G.S.

6. "On a new Species of *Loftusia* from British Columbia." By George M. Dawson, D.Sc., F.G.S., Assoc. R. S. M., of the Geological Survey of Canada.

June 19, 1878.—John Evans, Esq., D.C.L., F.R.S., Vice-President, in the Chair.

The following communication was read:—

1. "On the Section of Messrs. Meux & Co.'s Artesian Well in the Tottenham Court Road, with notices of the Well at Crossness, and another at Shoreham, Kent; and on the probable Range of the Lower Greensand and Palæozoic Rocks under London." By Prof. Prestwich, M.A., F.R.S., V.P.G.S.

The well-known boring at Kentish Town in 1856 showed the absence at that point of Lower Greensand, the Gault being immediately succeeded by hard red and variegated sandstones and clays, the age of which was at first doubtful, but which were finally considered by the author to approach most nearly to the Old Red Sandstone near Frome, and to the Devonian sandstones and marls near Mons, in Belgium. The existence of some doubt as to this identification rendered the boring lately made at Messrs. Meux's brewery particularly interesting; and the method of working adopted by the Diamond Boring Company, by bringing up sharply cut cores from known depths, gave special certainty to the results obtained. The boring passed through 652½ feet of Chalk, 28 feet of Upper Greensand, and 160 feet of Gault, at the base of which was a seam, 3 or 4 feet thick, of phosphatic nodules and quartzite pebbles. Beneath this was a sandy calcareous stratum of a light ash-colour, passing into a pale or white limestone, and this into a rock of oolitic aspect. Casts and impressions of shells found in this bed showed it to be the Lower Greensand, whose place it occupied. The boring was carried further in the hope of reaching the loose water-bearing sands of this formation; but the rock became very argillaceous; and

when 62 feet of it had been passed through, the boring entered into mottled red, purple, and greenish shales, dipping at 35° in an unascertained direction. These beds continued through a depth of 80 feet, when, their nature being clearly ascertained, the boring was stopped. The fossils of these coloured beds, which included *Spirifera disjuncta*, *Rhynchonella cuboides*, and species of *Edmondia*, *Chonetes*, and *Orthis*, show them to be of Devonian age. Thus the existence of Palæozoic rocks at an accessible depth under London, and the absence of the Jurassic series, as maintained long since by Mr. Godwin-Austen, are experimentally demonstrated.

These facts are of interest in connexion with the question of the possible extension of the Coal-measures under the Cretaceous and Tertiary strata of the south-east of England. The beds found at the bottom of Messrs. Meux's boring are of the same character as the Devonian strata which everywhere accompany the Coal-measures in Belgium and the north of France, being brought into juxtaposition with them by great faults and flexures. The author refers especially to a remarkable section at Auchy-au-Bois, in the western extremity of the Valenciennes coal-field, which is particularly interesting from its furnishing evidence that the Hardinghen coal-field, between Calais and Boulogne, is a prolongation of that of Valenciennes, and because the same strike and a prolongation of the same great fault observed at Auchy-au-Bois through Hardinghen would carry the southern boundary of any coal-field in the south-east of England just south of Maidstone, thence passing a little north of London. Hence it is in the district north of London that there is most probability of the discovery of the Carboniferous strata. The extent of country in which shafts could be sunk to the Palæozoic strata, however, will be limited by the presence of the water-bearing Lower Greensand, which probably reaches close to London in the south, reappears in Buckinghamshire and Bedfordshire 30 or 40 miles north of London, and probably extends some distance towards the city under the Chalk hills of those counties and Hertfordshire.

The nature of the representative of the Lower Greensand in the boring, and the characters of the fossils contained in it, lead the author to the conclusion that in it we have a deposit produced near the shore of the Neocomian sea, here probably consisting of cliffs of Devonian (or Carboniferous) rock. From these cliffs the calcareous material which here replaces the usual loose sands of the Lower Greensand was perhaps derived by the agency of springs; and the shore-line itself must be situated between the south end of Tottenham Court Road and the Kentish Town boring. The sandy beds of the Lower Greensand will probably be found to set in at no great distance to the southward, presenting the conditions necessary for storing and transmitting underground waters. A test boring made by Mr. H. Bingham Mildmay at Shoreham Place, about 5 miles from Sevenoaks, and in which the Lower Greensand was met with at about the estimated depth (450 feet) and furnished a supply of water, seems to confirm these views.

XXXI. *Intelligence and Miscellaneous Articles.*

ON SOME PROBLEMS OF THE MECHANICAL THEORY OF HEAT. BY PROFESSOR LUDWIG BOLTZMANN.

THE first section of the memoir has for its subject the relation between the Second Proposition and the calculation of probabilities; the second, the thermal equilibrium of a heavy gas. To these the author adds the following communication:—In the *Beiblätter* to Wiedemann's *Annalen der Physik*, Band II. Stück 5, is a treatise by S. Tolver Preston, in which the diffusion of gases is brought into relation with the Second Proposition of the mechanical theory of heat. This cannot (as, according to the notice in the *Beiblätter*, the author seems to think) serve for the refutation of that proposition; but it may well be a new and interesting application of it. There is in the same Stück of the *Beiblätter* a notice of a memoir by M. Clausius on this subject. Now, knowing nothing more of the contents of the memoir than what may be gathered from this notice, and not in the least wishing to forestall it, I will merely mention that, as it appears to me, the most essential problem that here comes into question, namely the calculation of how much heat can be transformed into work without any other compensation than the mixture of two dissimilar gases—or, to use the terminology of M. Clausius, the calculation of the transformation-value of the mixture of two dissimilar gases—is only a special case of some calculations which I have carried out in my treatise "On the Relation between the Second Proposition of the Mechanical Theory of Heat and the Calculation of Probabilities as regards the Propositions respecting Thermal Equilibrium." I have, namely, there considered quite generally the case that in any mixture of substances, whether it be the mixture or the distribution of velocities or of directions of velocities, the entropy does not perfectly correspond to the definitive final state, and have shown that then it must always be less than in the definitive final state, so that consequently, on the transition into the latter, heat may be converted into work: and I have given a formula (formula 51) by which the difference of entropy, and consequently also the amount of the convertible heat, can be calculated.

From this formula (51) it immediately follows that the entropy of a mixture of several gases is exactly equal to the sum of the entropies which would belong to the individual gases if, at the same temperature and under the same partial pressure, each were alone present in space. If V is the volume, T the absolute temperature, k the weight of the gas, c and c' its two specific heats, then its entropy (by which I understand $\int \frac{dQ}{T}$, dQ being = the heat introduced) is

$$k\int \frac{c\,dT}{T} + k(c'-c)\log V.$$

We will now consider two cases: first, two different gases are present in two different spaces V_1 and V_2, under equal pressure p

and at the same temperature T; secondly, the same gases are mixed, at the same temperature, in the space V_1+V_2, while the total pressure is equal to the previous pressure of each separate gas. In the first case let E_1 be the entropy of the first, E_2 that of the second gas; in the second case let $E_{1,2}$ be that of the mixture. According to the rule given above, E_1, E_2, and $E_{1,2}$ can be calculated by means of the above formula. We thus find

$$T(E_{1,2}-E_1-E_2)=T(\gamma'-\gamma)[(V_1+V_2)l(V_1+V_2)-V_1lV_1-V_2lV_2].$$

But this is the expression for the quantity of heat which can be converted into work without any other compensation than the mixture of the two gases. Therein is $\gamma'-\gamma$ the product of the weight of unit volume into the difference of the two specific heats, which product has the same value for both gases.

The total work which can be gained from this heat is, according to known principles,

$$p[(V_1+V_2)l(V_1+V_2)-V_1lV_1-V_2lV_2].$$

To gain this total work, of course we should not have recourse to the expedient of diffusion through porous partitions, but convey the one gas into the other by means of a substance that chemically combines with one of the gases under partial dissociation (as quick-lime with carbonic acid), of course taking care that the process always remains reversible. We should, for example, first indefinitely expand the first gas, then with the substance above mentioned transfer it very slowly into the other, while, again, it would be continually compressed so that the partial pressure of the first gas was always equal in both vessels. Lastly, the mixture of gases must be so far expanded that its volume shall be equal to the sum of the volumes of the original gases. Since all these processes can easily be accompanied by calculation, it will be easy in this way to verify the above-given formula.—*Kaiserliche Akademie der Wissenschaften in Wien, mathematisch-naturwissenschaftliche Classe*, June 6, 1878.

ON THE RELATION OF THE WORK PERFORMED BY DIFFUSION TO THE SECOND PROPOSITION OF THE MECHANICAL THEORY OF HEAT. BY PROF. R. CLAUSIUS.

In 'Nature' for January 1878 (vol. xvii. p. 202), Mr. Tolver Preston has specified a process by means of which mechanical work can be gained through diffusion of gases. The reflections he makes upon this fact are very ingenious, and in relation to theory very interesting on account of the conclusions to which they give occasion; only in one point I think I must express a view different from his: he thinks, namely, that the result of his process contradicts the second proposition of the mechanical theory of heat; and in this I cannot coincide.

The substance of his process is as follows. He imagines a cylinder divided into two sections by a movable piston. The piston consists of a porous substance, such as pipe-clay or graphite. In the two divisions of the cylinder two different gases are present, oxygen and hydrogen for instance.

If, now, both gases have initially equal pressure, a change is soon produced therein by diffusion. The hydrogen penetrates through the porous piston more quickly than the oxygen; hence the amount of gas present on the hydrogen side diminishes, while that on the oxygen side increases. This produces a lessening of pressure on the hydrogen side, and an augmentation of pressure on the oxygen side, so that the piston can be put in motion with a certain force, and mechanical work performed capable of being made useful externally. At the same time, with the movement of the piston the gas on the side where it expands becomes cooler, and becomes warmer on the side where it is compressed; and consequently heat passes over from a colder to a hotter body.

These two circumstances, that work is gained in the process without there being any difference of temperature present initially, and that simultaneously heat also passes over from the colder division into the hotter, are regarded by Mr. Preston as contradicting the second proposition of the mechanical theory of heat.

To this inference I cannot assent. If the conversion of heat into work and the transference of heat from the colder to the hotter body had taken place in such manner that *the variable material at the end of the operation were found in its original state, so that we had to do with a cyclical process*, then certainly there would be in it a contradiction to the second proposition of the mechanical theory of heat. But the matter does not stand thus. We have, in the process, as variable material the two gases. These are at the commencement unmixed, and at the conclusion mixed; and therefore an essential change has taken place with them, which may be regarded as a compensation for the conversion of heat into work and the transference of heat from a colder into a hotter body. Since the gases, through the molecular motion which we call heat, tend to mingle, and indeed in such wise that the higher the temperature the more quickly does the mixture result, we have here to do with an action of heat comparable to the expansion of a gas by heat; and hence we must ascribe to the mixed gases a greater disgregation than to the unmixed. Now, since the increase of disgregation is a positive change, it may be compensated by the transformation of heat into work and the transference of heat from a colder into a hotter body, both of which are negative changes.

It is therefore evident that, although it is true that the present case possesses certain peculiarities by which it is superficially distinguished from other cases, yet in the essential points with which we are concerned in the mechanical theory of heat it is in complete harmony with the cases usually treated, and contains nothing contradictory to the second proposition of the mechanical theory of heat.—Wiedemann's *Annalen*, 1878, pp. 341–343.

ON MOSANDRUM, A NEW ELEMENT. BY J. LAWRENCE SMITH.

Having read the interesting communication from M. J.-L. Soret to the Academy relative to the absorption-spectrum of the gadolinite

earths in the ultra-violet rays*, I hasten, with a view to my own interest, to bring to the knowledge of the Academy that the earth designated X was discovered by me more than a year ago: the discovery was publicly announced, in the course of the proceedings of the Philadelphia Academy of Natural Sciences, in May 1877†; a communication to the same effect was also sent to the said Academy in November of the same year.

My conclusions were based entirely on chemical principles; for it was proved that the earth which I had discovered was distinguished by its properties from all those known to belong to the yttria and cerium groups, although it came very near those earths, the chemical properties of which shade almost insensibly into one another. A short time after announcing the discovery, I sent a specimen to M. Delafontaine, of Chicago, who thought that it would prove to be either Mosander's terbia or some new earth. It was, however, impossible for me to make its properties agree with those at that time attributed to terbia.

I was desirous as far as possible to rid this new earth of the presence of earths already known, in order to study its properties and constituent parts. For better success I required a little of the terbia which M. Marignac had recently extracted from gadolinite. I wrote therefore, in March, to that able chemist. He possessed too small a quantity of that earth to let me have any; but he examined my new earth and the nitrate, which I sent to him; and in communicating to me the result of his examination he says (*inter alia*), "Not only am I convinced of the identity of your earth and my terbia, but I may add that you have obtained it in greater purity than I." After M. Soret had examined my earth by means of the spectroscope, he said to me, "I can have no doubt about the identity of that chemist's‡ [M. Delafontaine's] and my terbia and your earth."

The spectroscopic observation of M. Soret placed beyond doubt that the earths of samarskite contain a new metal, as I announced in May 1877, my first specimen then obtained giving the absorption-spectrum marked No. 2 in his communication. I no longer hesitate to give to this metal the name of *mosandrum*, in honour of the distinguished chemist whose researches and remarkable discoveries in this class of earths form a brilliant epoch in the history of metallic chemistry.

In giving the following succinct history of this discovery, I

* This has no relation to the Ural samarskite, in which I, concurrently with other chemists, have found oxide of cerium.

† *Comptes Rendus*, April 29, 1878.

"Professor Lawrence Smith made some observations on the anomalous properties of the earthy oxides of samarskite, and stated the reasons which led him to believe that those oxides do not contain cerium, and that most of what is regarded as cerium is a new element."—*Annals of the Philadelphia Academy of Natural Sciences*, May 8, 1877.

‡ In the course of more recent researches M. Delafontaine believed he had discovered another new earth: but M. Soret's experiments and M. Marignac's conclusions make it evident that this earth is identical with that discovered by me.

acknowledge my obligations to M. Delafontaine for the numerous suggestions with which he has kindly aided my investigations.

Towards the end of 1876 I was engaged in the mineralogical and chemical study of the American minerals containing niobic acid, and, among others, of samarskite, a considerable quantity of which had been found in North Carolina. In so doing I discovered two new minerals, a report on which was addressed to this Academy. In separating the earths from the samarskite I gained the conviction that they contained no acid oxide of *cerium*, or at the most only slight traces—a fact which is recorded in the publication of my results*. Other chemists who examined this samarskite (as Mr. Hunt, Mr. Allen, and Miss Swallow) concurred with each other in finding in it some oxide of *cerium*; M. Delafontaine, in a private letter dated 4th May 1877, writes to me, "I have ascertained nothing that can make me doubt the presence of *cerium*;" and in a letter of the 21st of the same month he passes in review the causes which might have misled me in my conclusions, ending with these words:—"But I suppose you have an equally good method: and we may expect to receive from you a monograph upon a new element—which, I avow, would give me great pleasure: for it has already seemed to me that the hypothesis of the existence of such an element would give a satisfactory explanation of certain incongruities in the properties of the other earths." In a still more recent letter he says, in regard to the samarskite earths, "I am convinced now of the absence, almost if not absolutely total, of the oxide of cerium: and no one has any longer a doubt on this point."

M. Delafontaine, to whom I remitted a little of the earth, which I had purified as much as it was then possible for me to do so, and also a large quantity of the mineral, concluded that it was either terbia or a new earth. Not finding, however, the chemical properties correspond to those then known of terbia, I addressed a report to the Paris Academy of Sciences, insisting on my first conclusions, asserting that the new earth is distinguished from that of the yttria group by the action of sulphate of potass†, from the oxide of cerium by its solubility in extremely dilute nitric acid and in a solution of alkalies supersaturated with chlorine, from lanthanum by the colour of its oxide and of its salts, from didymium by the absorption-lines of the latter in the bright part of the spectrum. I abstained from giving any definite name to the metal constituting the base of this earth, because I knew it was necessary to proceed with great circumspection, working, as I did, among a group of oxides which figure among the elements as the asteroids among the planets. But the spectroscope, in the skilful hand of M. Soret, has supplied what was wanting; and I seize this opportunity of announcing that the existence of the element, which I suspected in 1876, is no longer hypothetical, but real.—*Comptes Rendus de l'Académie des Sciences*, July 22, 1878, tome lxxxvii. pp. 148–151.

* *Archives des Sciences Physiques et Naturelles*, March 1878, p. 283.

† Being precipitated by a concentrated solution of that salt in the presence of crystals of the same salt, especially when heated, but less readily than the oxides of cerium, lanthanum, and didymium.

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[FIFTH SERIES.]

OCTOBER 1878.

XXXII. *On the Measurement of the Curves formed by Cephalopods and other Mollusks. By the* Rev. J. F. BLAKE, *M.A., F.G.S.**

[Plate IV.]

1. FORTY years ago Professor Moseley pointed out (Phil. Trans. 1838) that the curves affected by discoid and turbinated shells were derivable from the logarithmic spiral; and after him Naumann gave the name of concho-spiral to an allied curve, in which the differences of the radii in the same direction form a geometrical progression, but the initial radius is not one of the series. By such a modification he hoped to bring the measurements of actual shells more into harmony with calculation. The errors of observation, however, are always greater than this change would correct—if founded on fact, which is doubtful; and all practical advantage is lost by the complication of the equations.

In working through the fossil Cephalopoda, I have been obliged to obtain some clearness of idea as to their mode of origin and consequent connexion with each other, and to find some means of recognizing in fragments the nature of the whole; and I have thus been led to the following results.

2. If we examine the shell of a Nautilus in which the earlier are preserved by the later whorls, we find that the shell bends back upon itself as in fig. 1. Let A R B be the section of the earliest stage, which in the Nautilus is nearly a semicircle. On the growth of the shell, A B (the line in which the embryonic-

* Communicated by the Author.

shell section cuts the plane of growth) becomes D E, and D E subsequently G F. We have now only to suppose two laws of growth to make both inner and outer edges equiangular spirals. These are, that the shell-forming animal grows always obliquely to its present edge, and that the outside grows at a faster rate than the inside in a constant ratio. It is easy then to see, by similar triangles, that all the edges will meet in a common point C, which, when the growth becomes continuous, is the pole of the curve which the polygon becomes. If α be the angle between the radius and the tangent (in other words, represent the direction of growth), we may write the equation to the outer curve,

$$r = e^{\theta \cot \alpha}.$$

The initial radius is taken as unity for simplicity, the applications being always relative. The inner curve in the same way may be written

$$r = \lambda e^{\theta \cot \alpha},$$

or

$$r = e^{(\theta - \beta) \cot \alpha},$$

taking λ, the ratio of growth, to be $e^{-\beta \cot \alpha}$. This shows that we may consider the inner as an earlier portion of the outer curve, and look on its growth as retarded. The two elements, therefore, which completely determine the form of the section are α the spiral angle, and β the angle of retardation. It thus appears that the form of the central section of a Nautilus is independent of the shape and size of the embryo, depending only on the direction and relative rate of growth.

If, now, we consider turbinated shells, a new element is introduced. The direction of growth is no longer in one plane, but makes a constant acute angle with a fixed plane ; and this, in combination with the variable angle which the direction of growth makes with a fixed line in that plane, produces a curve of double curvature, which, as it partakes of the nature both of a helix and a spiral, may be called a helico-spiral. This third constant angle, which will be the complement of the semi-vertical angle of the enveloping cone for a fixed point in the surface of growth, may be called the angle of elevation, and be denoted by γ.

These three angular elements, together with the equation to the trace on any plane through the pole making a finite angle with the direction of growth thus determined, of the outline of one whorl of the shell, are sufficient completely to determine the form of the whole shell so long as the growth is uniform; and these, therefore, or their equivalents ought to form

Fig. 8.

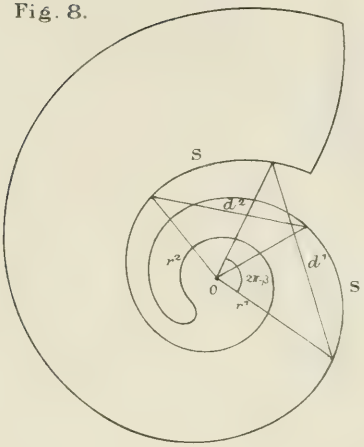


Fig. 9.

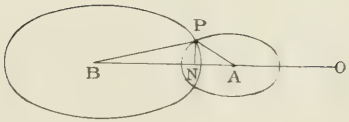
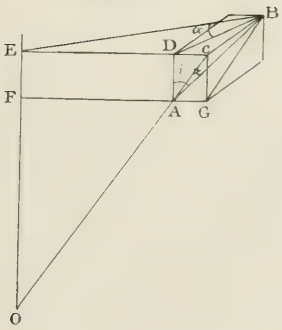


Fig. 10.



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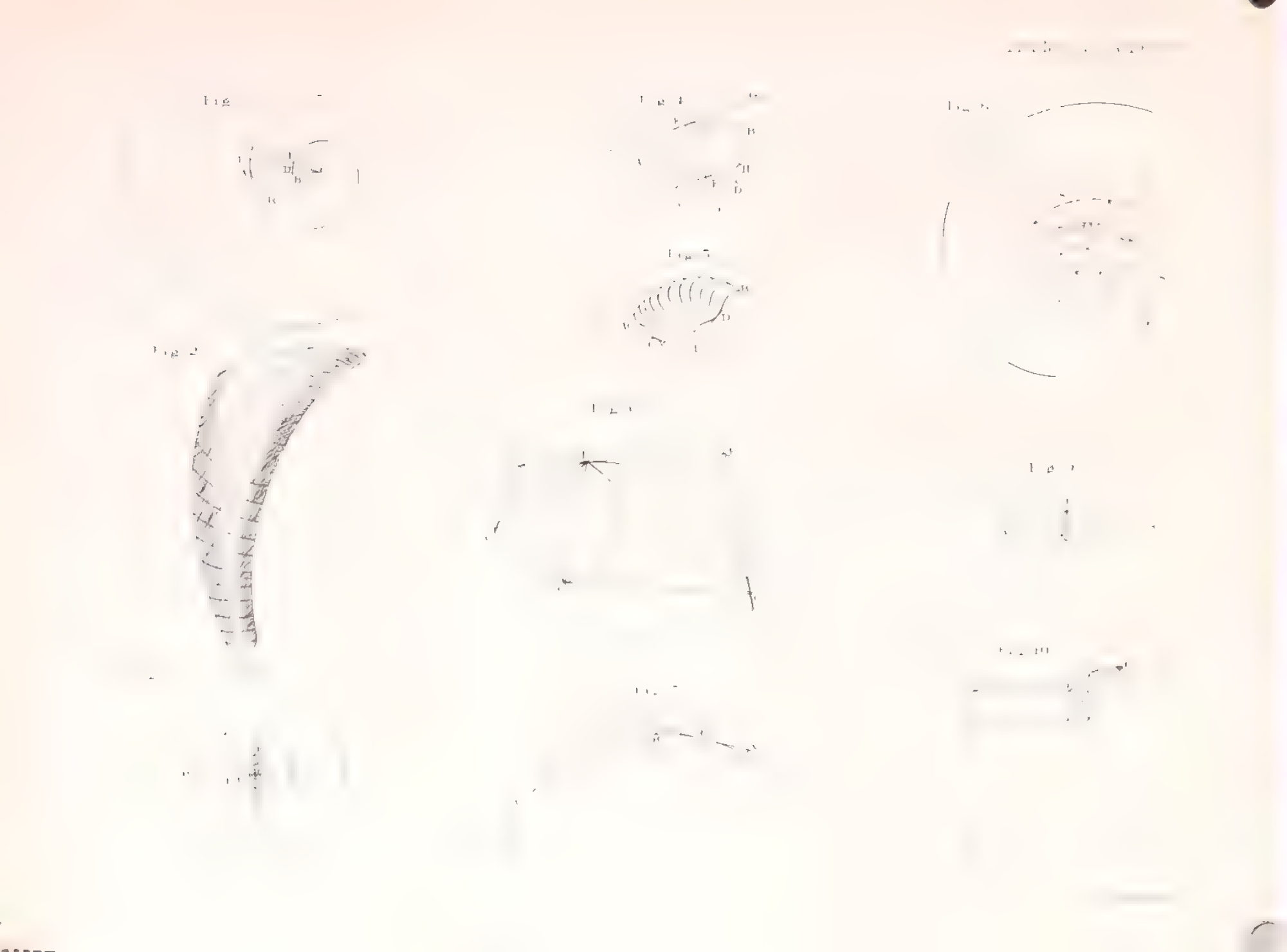


part of the description, when complete, of every regular molluscan shell.

3. As an example, suppose the trace elliptical, which is very often the case.

To find the equation to the surface of a turbinated shell of elliptical section.

Taking the pole as origin, and the axis of the shell as that of z, let λ, μ be the radial and vertical coordinates of the centre of any one of the elliptic sections; a, b the semiaxes; ϵ the angle the major axis makes with that of z. Then the equation to the ellipse is

$$\frac{\{(z-\mu)\cos\epsilon+(r-\lambda)\sin\epsilon\}^2}{a^2}+\frac{\{(r-\lambda)\cos\epsilon-(z-\mu)\sin\epsilon\}^2}{b^2}=1.$$

Taking the centre as the point of reference, if γ be the angle of elevation,

$$\mu=\lambda\tan\gamma;$$

and if α be the spiral-angle,

$$\lambda=\epsilon^{\theta\cot\alpha}.$$

Also, if β be the angle of retardation of the inner end of the major axis behind the centre,

$$a=\{e^{\theta\cot\alpha}-e^{(\theta-\beta)\cot\alpha}\}\operatorname{cosec}\epsilon=e^{\theta\cot\alpha}(1-e^{-\beta\cot\alpha})\operatorname{cosec}\epsilon$$
$$=e^{\theta\cot\alpha}\sigma\operatorname{cosec}\epsilon,\text{ say.}$$

Putting $b=\kappa a$ and making the above substitutions, the equation to the surface becomes

$$\kappa^2\{(z-e^{\theta\cot\alpha}\tan\gamma)\cos\epsilon+(r-e^{\theta\cot\alpha})\sin\epsilon\}^2$$
$$+\{(z-e^{\theta\cot\alpha}\tan\gamma)\sin\epsilon-(r-e^{\theta\cot\alpha})\cos\epsilon\}^2$$
$$=\kappa^2\sigma^2\operatorname{cosec}^2\epsilon\,.\,e^{2\theta\cot\alpha},\quad\ldots\ldots\quad(\Lambda)$$

which may be immediately transformed into one between x,y,z by the substitution of x^2+y^2 for r^2, and $\tan^{-1}\frac{y}{x}$ for θ, or into a polar one by substituting $\rho\cos\phi$, $\rho\sin\phi$ for z and r; which will bring it into harmony with the more general functional but unmanageable equation given by Professor Moseley. In most cases, except among the Gasteropoda, ϵ may be assumed $=90°$.

4. By giving different values to the constants in this equation we can obtain all the varieties of shells of elliptical section, and in a similar manner those of any other form of section. Thus for all the Brachiopoda the value of γ is zero. In both these and the Lamellibranchiata σ is never much less than unity, and often greater; and at the same time α is small

and equal in the two valves among the Lamellibranchiata, but greater in the ventral valve among the Brachiopoda. The various forms among the latter class are produced partly by changes of form of the tracing curve, partly by differences in the value of σ. Thus such transversely oval forms as *Obolus* have approximately $\kappa > 1$, and the inner edge coinciding with the axis, or $\sigma = 1$; while elongate ovals, as *Siphonotreta*, have $\kappa < 1$. Where the hinge-line is straight, as in *Spirifera* and other genera, the ellipse, in which usually $\kappa > 1$, overlaps the axis, and consequently $\lambda < a$, which involves $\sigma > 1$; to obtain which the angle of retardation must be imaginary, as it must be therefore in all shells in which we assume a curve cutting the axis. Then also the ratio of growth of the inner edge, which does not, so to speak, exist, is imaginary; but σ, with which we have practically to deal, is real. We can in these cases nevertheless speak of the angle of retardation as being that of the centre over the outer edge, unless the centre itself is on the negative side of the axis, when the phrase becomes useless and σ is negative and > 1. In such conical forms as *Discina* α is nearly zero, and γ has a moderately small positive or negative value, the latter corresponding to the flat valve. When, as is usually the case, the two valves turn in opposite directions, we must reckon α negative in the dorsal valve; but when that valve is concave, as in the *Productidæ*, α is still positive, but has a different value, less than that of the ventral.

In the Lamellibranchiata these same differences are combined with a very small but not zero value of γ. They all, with few exceptions, principally among the *Ostreidæ*, have the values of α of opposite sign in the two valves, and they present a greater variety of tracing curves. Their values of α, however, are larger than we find among the Brachiopoda, whose maximum values belong to the *Pentameri*. Of this we have notable examples, combined with a considerable value of γ, in such shells as *Isocardia* and *Diceras*, and with a very low value of γ in *Caprinella*.

Among the Gasteropoda (for a considerable number of which the value of α has been observed by Professor Naumann), we have $\alpha = 0$ for the limpets, while β is very small for the *Dentaliadæ* and very large for *Haliotis*. The suitable values for the other mollusks (the Pteropoda and Cephalopoda) will immediately suggest themselves—as we may say generally that the curvature depends on α, the involution on β, and the elevation on γ.

The direction of the major axis of the generating ellipse in Gasteropoda is generally oblique—being approximately parallel

to the opposite slope of the shell in those with a small apical angle, and parallel to the adjacent slope when the vertical angle is large, becoming parallel to the axis when $\gamma=0$. It has also the same direction in *Turrilites*.

5. *To find the conditions that the whorls should intersect on the same or on opposite sides of the axis.*

In equation (A) put $z=r\tan\gamma$, whence

$$(r-e^{\theta\cot\alpha})\sqrt{\kappa^2(\tan\gamma\cos\epsilon+\sin\epsilon)^2+(\tan\gamma\sin\epsilon-\cos\epsilon)^2} = \pm\kappa\sigma e^{\theta\cot\alpha}\operatorname{cosec}\epsilon,$$

or

$$r=e^{\theta\cot\alpha}\left\{1\pm\frac{\kappa\sigma\operatorname{cosec}\epsilon\cos\gamma}{\sin(\epsilon+\gamma)\sqrt{\kappa^2+\cot^2(\epsilon+\gamma)}}\right\}.$$

If the whorls touch on the same side of the axis, the larger of these values must equal what the smaller becomes when $\theta+2\pi$ is written for θ—if they cut, greater—if out of contact, less, since the similarly placed ellipses must touch if they meet on the line joining their centres. The whorls therefore intersect or not according as

$$1+\frac{\kappa\sigma\operatorname{cosec}\epsilon\cos\gamma}{\sin(\epsilon+\gamma)\sqrt{\kappa^2+\cot^2(\epsilon+\gamma)}} > \text{or} < e^{2\pi\cot\alpha}\left\{1-\frac{\kappa\sigma\operatorname{cosec}\epsilon\cos\gamma}{\sin(\epsilon+\gamma)\sqrt{\kappa^2+\cot^2(\epsilon+\gamma)}}\right\},$$

as

$$\frac{\kappa\sigma\operatorname{cosec}\epsilon\cos\gamma}{\sin(\epsilon+\gamma)\sqrt{\kappa^2+\cot^2(\epsilon+\gamma)}} > \text{or} < \frac{e^{2\pi\cot\alpha}-1}{e^{2\pi\cot\alpha}+1}. \quad . \quad (1)$$

If corresponding whorls on the opposite sides of the axis touch, the greater value of z when $r=0$ in equation (A) must equal the smaller value of z when $\theta+\pi$ is written for θ, and r again put equal to 0, since it is easily shown that if the two ellipses meet on the axis they must touch. The condition for this may be deduced in a similar manner to the last; and we find accordingly that shells are umbilicated or not according as

$$\{(\kappa^2\cot^2\epsilon+1)\tan\gamma+\cot\epsilon(\kappa^2-1)\}(e^{\pi\cot\alpha}-1) > \text{or} < \operatorname{cosec}^2\epsilon\,\kappa\sqrt{\sigma^2(\kappa^2\cot^2\epsilon+1)-1}(e^{\pi\cot\alpha}+1). \quad (2)$$

Such an umbilicus, however, will be spiral; a straight one, caused by the generating curve not meeting the axis, will exist when the radical is impossible, *i. e.* when

$$\sigma\sqrt{\kappa^2\cot^2\epsilon+1}<1.$$

Very few shells make the expressions above compared equal to each other. *Cyclostoma* and *Scalaria* are examples in which this nearly happens in both cases. Some *Turritiæ* also approximate to making (1) an equality, and (2) somewhat less closely.

As an example of the application of these equations, the shell of *Cyclostoma elegans* may be taken. In this the value of γ, ascertained by measurement, is $78\frac{1}{2}°$, $\epsilon = 156°$, $\kappa = \cdot 87315$, and σ may be calculated, either directly from the equation defining it or by methods to be presently noted, to be ·46. Also $e^{2\pi \cot \alpha}$ by observation $= \frac{14}{9}$. Substituting these values in

$$\frac{\kappa\sigma \operatorname{cosec} \epsilon \cos \gamma}{\sin(\epsilon+\gamma)\sqrt{\kappa^2 + \cot^2(\epsilon+\gamma)}},$$

we obtain ·216 approximately, while

$$\frac{e^{2\pi \cot \alpha} - 1}{e^{2\pi \cot \alpha} + 1} = \cdot 217.$$

The whorls therefore, according to this calculation, are slightly out of contact; and it will be seen in the shell that the outline deviates slightly from the elliptic form in order to bring them into contact. Similar substitutions in the expression of (2) make the left-hand side 2·679, while the left is ·841. The whorls therefore cut the axis, but leave a spiral umbilicus.

6. In the case of discoid shells we need only discuss the condition of contact on the same side of the axis. Here $\gamma = 0$ and $\epsilon = 90°$, and (1) becomes

$$\sigma > \text{ or } < \frac{e^{2\pi \cot \alpha} - 1}{e^{2\pi \cot \alpha} + 1},$$

showing that contact or overlapping is independent of the shape of the whorls. Indeed it is obvious that they will be in contact or not according as the retardation of the inner edge is greater or less than 360°. As the retardation increases from this value the whorls more or less overlap, becoming completely involute when β is infinite, in which case $\sigma = 1$. When the axis is cut by the whorls, as in the case of the *Nautilus*, whose shape is best represented by half an ellipse rapidly rounded into the umbilicus, the inner edge of the major axis becomes non-existent, β being imaginary. It is by the variation of this element alone that such genera as *Crioceras* and *Gyroceras* are separated from Ammonites and *Nautilus*. They are therefore less distinct than *Turrilites* and *Toxoceras*, which differ in addition by the whorls of the first always cutting

the axis. The genus *Trochoceras* indeed lies between them in this respect.

7. Similar methods to the above may be adapted to other generating curves besides the ellipse. For the circle, of course, we have only to put $\kappa = 1$. In many cases no simply expressed equation can represent the shape; but there are a few that do admit of this. Thus, in the Ammonites of the group *Cordati* the curve is nearly represented by the cardioid whose equation is

$$\rho = a(1 + \cos \phi).$$

Here the retardation of the pole from the apex being

$$2a = e^{\theta \cot \alpha}(e^{\beta \cot \alpha} - 1) = 2\sigma e^{\theta \cot \alpha} \text{ say, and } \tan \gamma = 0,$$

the equation to such an Ammonite will be

$$\{(r - e^{\theta \cot \alpha})(r - (1+\sigma)e^{\theta \cot \alpha}) + z^2\}^2 = \sigma^2 e^{2\theta \cot \alpha}\{(r - e^{\theta \cot \alpha})^2 + z^2\}.$$

8. It is to be noted that, if a plane section through the middle point of growth have an elliptical or any other curved outline, the trace on any other plane through the same point will not be of the same character; and though the assumption (say) of an elliptical section in a plane perpendicular to the direction of growth of the middle point may be as near an approximation to nature as the one chosen, and the corresponding equation may be worked out, yet the results are quite unmanageable, and have no particular relation to the laws of growth already enunciated. The curve assumed in the above investigation is that exposed by a longitudinal section of the shell through its axis. It has therefore no necessary connexion with the form of the aperture, which may be in another plane, or, indeed, have a curve of double curvature for its outline.

This difference is particularly to be noticed in conical shells, such as the *Orthoceras*, which correspond to the value $\alpha = 0$. In these we cannot take a plane through the pole (*i. e.* the apex) as the surface of growth. Nor can we consider one side more retarded than the other. Fundamental differences are thus revealed between the laws of growth of such shells and those which are curved. We may, however, connect them both theoretically and by natural links. To turn an equiangular spiral about its pole through a given angle is to bring an earlier point in the curve into any particular radius; so that the apical angle of a conical shell corresponds to the angle of retardation in a discoid one: only in the latter case the inner curve stops on the extreme radius of the outer, while in the former it is continued to be of the same length. In some *Cyrtocerata* and in the opercula of Gasteropods we may

have a curved shell (see fig. 2) in which the ornaments approximately run at a constant distance from the pole, while the septa approximate to a radial direction. Thus one law of growth is illustrated by the inside and another by the outside.

9. The several elements of the shell have hitherto been considered constant; the results of their variation may now be indicated. The rapid increase of α produces such shells as *Œkotraustes*, a subgenus of *Ammonites*; and the rapid diminution to zero, *Scaphites*, *Ancyloceras*, and *Lituites*. Its more gradual increase with age is shown in *Pupa* and some *Bulimi*; and its diminution in many *Orthocerata*, which are curved in youth. An increase of β alone produces a more involute shell, as many *Goniatitæ* and *Bellerophon expansus*—and in combination with a decrease of α, shells such as *Succinea* and *Sigaretus*. Its decrease contracts the body-chambers of many *Orthocerata*. An alteration of γ, combined in some cases with an alteration of α, takes place in all those Gasteropods whose whorls cannot all be touched by the same cone, and whose spire may be called concave or convex. As the former is the the commoner of the two, it follows that γ most often decreases with age. The changes in *Vermetus* and *Siliquaria* seem to be brought about by an increase in γ alone. Its rapid decrease, on the contrary, bringing it to a negative value, produces the depressed spires of some Cones and of the *Cyprææ*. When all three vary simultaneously, the effects may be so masked as to be ascertainable only by careful measurement.

In the Cephalopoda, which have part of their shells partitioned off, some authors give among the characters both the length and the capacity of the body-chamber. These two are evidently deducible from each other, whatever may be the shape of the shell, provided its growth continues constant; for if the length of the body-chamber $=(1-\kappa)$ whole length, the capacity $=(1-\kappa^3)$ whole capacity: if therefore it can be measured independently, it will show if any contraction or expansion has taken place.

It remains now to show how the three elements of shells may best be observed, and especially on imperfect specimens such as fossils very commonly are.

10. *To find the spiral angle of a discoid or turbinated shell.*

Since in a turbinated shell all the radii from the apex and arcs of the curve lying on one cone are multiples of their projections on the plane perpendicular to the axis, we need only consider discoid forms at first. Let A B C D (fig. 3) be such a form; A C, B D two diameters at right angles through its pole. Then, from the properties of the equiangular spiral, it is obvious that we have a choice of several methods of determining α, by

which results may be checked, or the angle ascertained on small fragments of the shell; and since the outer and inner curves have the same angle, observations on either will suffice. Thus $e^{\pi \cot \alpha}$ is given by any of the following ratios:—

$$\frac{AO}{OC'}, \frac{AE}{GC'}, \frac{EO}{OG'}, \frac{EK}{MG};$$

so $e^{2\pi \cot \alpha}$ is gven by $\frac{EK}{EN'}$, a value chiefly useful for fragments; and $e^{\frac{\pi}{2}\cot \alpha}$ by

$$\frac{AC}{BD'}, \frac{AE}{HD'}, \frac{EG}{FH'} \text{ or } \frac{MG}{FL}.$$

The first of these series gives the best result on unornamented shells, and the second on ornamented. To facilitate the obtaining the angle from the observed ratios, the following Table of solutions of the equation $\cot \alpha = \frac{\log_e R}{\pi}$ has been prepared, which may at once be applied to the others by squaring or extracting the root:—

Table of Solutions of $\cot \alpha = \frac{\log_e R}{\pi}$.

R.	α.	R.	α.	R.	α.	R.	α.
1·01	89° 49′	1·26	85° 48′	1·51	82° 32′	1·76	79° 48′
1·02	89 38	1·27	85 39	1·52	82 24	1·77	79 42
1·03	89 28	1·28	85 30	1·53	82 17	1·78	79 36
1·04	89 17	1·29	85 22	1·54	82 10	1·79	79 30
1·05	89 7	1·30	85 14	1·55	82 3	1·80	79 24
1·06	88 56	1·31	85 5	1·56	81 57	1·81	79 18
1·07	88 46	1·32	84 57	1·57	81 50	1·82	79 12
1·08	88 36	1·33	84 49	1·58	81 43	1·83	79 7
1·09	88 26	1·34	84 41	1·59	81 36	1·84	79 1
1·10	88 16	1·35	84 33	1·60	81 29	1·85	78 55
1·11	88 6	1·36	84 25	1·61	81 23	1·86	78 49
1·12	87 56	1·37	84 17	1·62	81 16	1·87	78 44
1·13	87 46	1·38	84 9	1·63	81 10	1·88	78 38
1·14	87 37	1·39	84 1	1·64	81 3	1·89	78 33
1·15	87 27	1·40	83 53	1·65	80 57	1·90	78 27
1·16	87 18	1·41	83 45	1·66	80 50	1·91	78 22
1·17	87 8	1·42	83 38	1·67	80 44	1·92	78 16
1·18	86 59	1·43	83 30	1·68	80 37	1·93	78 11
1·19	86 50	1·44	83 23	1·69	80 31	1·94	78 5
1·20	86 41	1·45	83 15	1·70	80 25	1·95	78
1·21	86 32	1·46	83 8	1·71	80 18	1·96	77 54
1·22	86 23	1·47	83	1·72	80 12	1·97	77 49
1·23	86 14	1·48	82 53	1·73	80 6	1·98	77 44
1·24	86 5	1·49	82 46	1·74	80	1·99	77 39
1·25	85 56	1·50	82 39	1·75	79 54	2	77 33

When the ratio is > 2, we generally have to deal with evolute

shells, in which the angles of the different species do not lie so close together; and the following inverse Table will be sufficient to check the direct measurement of the angle by the observation of ratios:—

Table of Solutions of $R = \epsilon^{\pi \cot \alpha}$.

α.	R.	α.	R.	α.	R.
89°	1·06	69°	3·25	49°	11·8
88	1·12	68	3·44	48	12·7
87	1·18	67	3·66	47	13·7
86	1·25	66	3·88	46	14·8
85	1·32	65	4·12	45	16·0
84	1·39	64	4·38	44	17·3
83	1·47	63	4·66	43	18·8
82	1·56	62	4·95	42	20·4
81	1·65	61	5·27	41	22·1
80	1·74	60	5·62	40	24·0
79	1·84	59	5·99	39	26·3
78	1·95	58	6·39	38	28·7
77	2·05	57	6·82	37	31·5
76	2·16	56	7·28	36	34·6
75	2·29	55	7·78	35	38·0
74	2·43	54	8·32	34	42·0
73	2·58	53	8·92	33	46·4
72	2·73	52	9·55	32	51·4
71	2·89	51	10·3	31	57·3
70	3·06	50	11·0	30	64·0

11. These methods suffice for recent or complete shells; but sometimes a fragment of a single whorl is all that is presented to observation, in which case there are two or three possible methods.

1st. Suppose the inner as well as the outer edge of the whorls preserved, as A B, C D (fig. 4). Then, if we draw any two parallel tangents, as E G, F H, the straight line joining the points of contact must pass through the pole. Hence G E F is the angle required. And even if neither edge be preserved, any pair of longitudinal striæ or other ornaments will suffice for the purpose.

2nd. Let the directions and lengths of a pair of transverse ornaments, which we may assume to make the same angle with the radius, be available (fig. 5) as A C, B D. Then the angle between A C, B D equals the angle between the radii, $=\theta$ say: we have therefore $\frac{BD}{AC} = e^{\theta \cot \alpha}$, which gives α in terms of known quantities.

3rd. Let the outer edge alone be available. Here we have the simple problem, Given an arc of an equiangular spiral, to find its pole and spiral angle. This admits of two easy solutions:—

(1) Let two tangents D B, DA (fig. 6) be drawn to the curve. Then, if O be the pole, since O A D, O B E are equal, a circle described round D B A will pass through the pole: this point may thus be determined by the intersection of two or more such circles and the angle $\alpha = \text{D B O}$ observed. This method, though theoretically simple, requires so much accuracy in determining the actual points of contact of the tangents as to be generally inapplicable.

(2) If ρ_1, ρ_2 be the radii of curvature at two points of an equiangular spiral, and s the arc between them, then

$$\cot \alpha = \frac{\rho_1 - \rho_2}{s}.$$

Now the value of ρ_1 at A can be very closely approximated to by measuring the distance AC at which a constant small offset CD is made by the curve; and this accuracy may be increased if the offset be small enough by placing it on both sides of the point of contact, and assuming the point to be midway between its two positions. If $AC = x_1$, and the offset μ,

$$\rho_1 = \frac{x_1^2 + \mu_1^2}{2\mu} \text{ approximately;}$$

whence, if x_2 be the corresponding distance of the same offset when the tangent is at B,

$$\cot \alpha = \frac{x_2^2 - x_1^2}{2\mu s}.$$

If $2\mu s$ be chosen to be 100 units, the calculation is very simple, and gives results closely concordant with those derived from the complete shell.

12. These formulæ may be exemplified on *Ammonites obtusus*. Two diameters at right angles measured 224 and 188 millims. respectively, whence $e^{\frac{\pi}{2}\cot\alpha} = 1{\cdot}19$, or $R = 1{\cdot}41$. Two others measured 208 and 176 millims., whence $R = 1{\cdot}39$. The breadths of two whorls along the same diameter measured 76 and 56 millims., whence $R = 1{\cdot}39$. Two others were 64 and 46 millims., whence $R = 1{\cdot}39$. Two parallel tangents were drawn and the points of contact joined; these made an angle of $83\frac{1}{2}^\circ$ with the tangents when care was taken that the tangents should be at similar points, the outline of this Ammonite being slightly polygonal. With the same precaution, an offset of 5 millims. was $35\frac{1}{2}$ millims. on each side of one point of contact: at another, distant 187 millims. of arc, measured by carefully rotating the shell along the ruler, the same offset was $32\frac{1}{2}$ millims. on each side; whence $\cot \alpha = 1{\cdot}079$, whence $\alpha = 83^\circ\ 50'$. Extracting from the table the angles corresponding to $R = 1{\cdot}39$

and R = 1·41 and taking the mean, we find the angle $\alpha = 83° 52'$. This example will illustrate the amount of coincidence that may be hoped for from careful measurements.

13. All these methods lead, of course, to erroneous results when the shell has suffered distortion, except those derived from measures taken along a single straight line. For example, the value derived from the ratio $\frac{AE}{GC}$ ought to give the same result as $\frac{HD}{BF}$ (fig. 3), whatever the distortion: but these ratios will not be the square of $\frac{AE}{HD}$. If the shell has been elongated in the direction AC in the ratio $x : 1$, we have

$$\frac{AE^2}{x^2 . HD^2} = e^{\pi \cot \alpha} = \frac{AE}{GC},$$

whence

$$x = \sqrt{\frac{AE}{HD}} \sqrt{\frac{GC}{HD}},$$

which gives a measure of the distortion.

An example of this may be taken from specimens of *Goniatites*, which are often distorted, in which state they have been called *Ellipsolithes*. Four whorl-breadths at right angles were measured at 20, $17\frac{1}{2}$, $14\frac{1}{2}$, $12\frac{3}{4}$ lines respectively, whence $\frac{AE}{GC} = 1·378$, $\frac{HD}{BF} = 1·372$ (fig. 3), the difference being due to errors of observation on ill-preserved shells. The mean may be taken as 1·375 for the true ratio. The values of the ratio derived from squaring the ratios $\frac{AE}{HD}$, $\frac{HD}{GC}$, $\frac{GC}{BF}$, respectively are 1·306, 1·456, 1·293. The first and third of these differ only from errors of observation: but their variation from the true ratio is such as would be produced by distortion, the value of which is either $\sqrt{\frac{1·306}{1·456}} = ·947$ or $\sqrt{\frac{1·293}{1·456}} = ·942$: that is, the diameter of the shell in the direction AC has been diminished by compression in the ratio :94.

14. The value of the angle of retardation depends, of course, on the two points assumed. When the section is an ellipse, and the centre and extremity of major axis are compared, it depends on the length and position of the major axis. But in the more general case, whatever be the shape of the curve, especially if the whorls intersect, and in discoid shells, it is

more convenient to compare the outer edge with the intersection of the whorl with the one next to it inside.

To determine the angle of retardation of the inner edge behind the outer in a discoid shell.

This element is given by the ratio of the umbilicus and of the outer whorl to the diameter. If λ and μ be these ratios,

$$\lambda = e^{-\beta \cot \alpha}, \quad \mu = \frac{1-e^{-\beta \cot \alpha}}{1+e^{-\pi \cot \alpha}};$$

whence

$$\beta \log \mathrm{R} = -\pi \log \lambda \text{ and } \mathrm{R} = \frac{\mu}{1-\lambda-\mu},$$

or

$$\beta = \frac{\pi \log \lambda}{\log (1-\lambda-\mu) - \log \mu}.$$

In the description of Cephalopoda we often meet with such an expression as "inner whorls two thirds concealed." If the inner whorl be concealed in the ratio of $x : 1$,

$$x = \frac{e^{-2\pi \cot \alpha} - e^{-\beta \cot \alpha}}{e^{-2\pi \cot \alpha} - e^{(-\beta-\pi) \cot \alpha}} = \frac{1-\mathrm{R}^2\lambda}{1-\mathrm{R}\lambda};$$

whence, if R is known, λ and then β may be deduced; which might sometimes be convenient in the case of a fragment. In this case, however, another method may be followed. If b_1 and b_2 be the lengths of two lines measured in the direction of the radii, and s the arc of the outer curve between them, then

$$b_1 - b_2 = s \cos \alpha (1 - e^{-\beta \cot \alpha}).$$

As an example *Ammonites margaritatus* may be taken. In one diameter of the septate portion of the shell, of length 166½ millims., the umbilicus was 51 and the last whorl 72; this gives $\lambda = \cdot 307$ and $\mu = \cdot 432$. In another perpendicular diameter of 131 millims. the umbilicus was 42½ and the last whorl 55; whence $\lambda = \cdot 324$ and $\mu = \cdot 41$. The values of R deduced from these are 1·65 and 1·64. The decrease in the value of λ and the corresponding increase of μ shows that this species grows more involute with age; in other words, β varies. In the earlier whorls it is 820°. It might be thought that Professor Naumann's conchospiral would in this instance produce closer approximations to the observed values; but the value of R deduced from the radii at right angles is 1·69; whereas it ought to be less than that derived from whorl-breadths, if the inner and outer curve were both conchospirals.

Another example taken was *Turrilites scheutzerianus*. Here the diameter at the last whorl was 56 millims., the penultimate diameter 43 millims.; the last whorl had a horizontal breadth

of 29 millims., being a vertical ellipse. In this case the umbilicus, not being approximately in one plane, has to be calculated: we obtain R=1·378, whence

$$\lambda=1-\frac{29}{56}\left(1+\frac{1}{1\cdot378}\right),$$

$$\beta=\pi\frac{\log\lambda}{-\log R}=\pi\frac{1\cdot673713}{\cdot05626}=29\cdot66\pi;$$

or the inner edge of the shell represents the surface nearly fifteen whorls behind.

The other two formulæ may be illustrated on *Ammonites lævisculus*. The measurements along one diameter of 50 millims. were—outer whorl $21\frac{1}{3}$, umbilicus 14, remainder $14\frac{2}{3}$; whence R=1·47 and $\lambda=\cdot28$. Substituting these values, we obtain $x=\dfrac{1-\cdot6050}{1-\cdot4116}=\dfrac{3950}{5884}=\cdot67$. From actual measurement the whorls are $\frac{27}{41}$ concealed, *i. e.* ·66. The value of α corresponding to R=1·47 is 83°. The breadths of the whorls at two points distant 67 millims. of arc are $14\frac{1}{3}$ and 20 millims. respectively; whence

$$5\cdot6667=67\times\cdot1218\ (1-\lambda),$$

which gives $\lambda=\cdot32$. This latter measurement is the least trustworthy, especially if the direction of the radii cannot be accurately ascertained.

15. In turbinated shells whose elliptic axis is oblique, we require to know the retardation of the inner extremity of the major axis, *i. e.* the value of σ. This is best done by comparing the diameter perpendicular to the axis with the major axis of the last whorl. To determine this diameter we must add the two radii obtained from the condition that z shall have equal roots in the equation A which correspond to θ and $\theta-\pi$. This condition gives us

$$r=e^{\theta\cot\alpha}(1\pm\sigma\sqrt{\kappa^2\cot^2\epsilon+1}).$$

The smaller of these values corresponds to the radius, if any, of the open umbilicus, as in § 5; the larger is the one we require. Therefore, if d be the measured diameter,

$$d=e^{\theta\cot\alpha}(1+e^{-\pi\cot\alpha})\left\{1+\sigma\sqrt{\kappa^2\cot^2\epsilon+1}\right\};$$

and since $a=e^{\theta\cot\alpha}\sigma\operatorname{cosec}\epsilon$,

$$d\sigma\operatorname{cosec}\epsilon=a\left\{1+e^{-\pi\cot\alpha}\right\}\left\{1+\sigma\sqrt{\kappa^2\cot^2\epsilon+1}\right\}.$$

For example, in the *Cyclostoma elegans* before mentioned,

$d=12$ millims. and $a=3{\cdot}7$; also

$$\kappa={\cdot}87315, \quad \epsilon=156°, \quad \mathrm{R}=\sqrt{\frac{14}{9}};$$

$$\therefore\ 12\times 2{\cdot}4586\,\sigma=3{\cdot}7\times 1{\cdot}80178\left\{1+\sigma\,2{\cdot}2216\right\};$$

$$\therefore\ 14{\cdot}6926\,\sigma=6{\cdot}6666, \text{ and } \sigma={\cdot}46.$$

The value of σ may also be calculated from an equation, to be given hereafter, for determining the angle of elevation γ from the angle of the tangent cone, whenever the value of γ can be measured directly.

16. In the case of the conical shells of the *Orthocerata*, in which we can no longer speak of the angle of retardation, but must measure the apical angle of the cone, the above methods are not applicable. Here, however, the apical angle can always be directly observed on the sides of the shell: or if it have a known number of longitudinal ornaments, by taking that multiple of the angle between two of them and dividing by 2π we obtain the sine of half the required angle if the section be circular. It may also be obtained for any particular plane, whatever be the section, by the formula

$$\frac{d_1-d_2}{2s}=\tan\frac{\beta}{2} \text{ or } \sin\frac{\beta}{2},$$

if d_1, d_2 be two diameters in that plane, and s the distance between them measured either on the axis or on the slant side. It would be unnecessary to remark that it is the difference and not the ratio of the diameters which gives us the apical angle, were it not that this ratio is given by some palæontologists among the shell-characters, where of course it is perfectly useless. Methods quite analogous may be used for curved shells where the pole is known. For if with pole as centre any circle be described cutting the two curves, the angles between the tangents or between the radii corresponding to the points of intersection will be the β required. Or if d_1, d_2 are the distances between these points for two circles,

$$\frac{d_1-d_2}{2(r_1-r_2)}=\sin\frac{\beta}{2}, \text{ and } \frac{d_1-d_2}{2s}=\sin\frac{\beta}{2}\,.\cos\alpha,$$

r_1, r_2, s being taken on either curve (see fig. 8).

If the transverse section of a conical shell is elliptical, the apical angle will not, of course, be the same in the planes of the major and minor axis, though, the ratio of these being constant, one angle may be deduced from the other. When curvature takes place in the plane of the major axis, the curves formed by the ends of the minor axis do not lie in one plane,

but on a cone whose vertical angle 2δ is given by $\cot\delta=\kappa\frac{1+\lambda}{1-\lambda}$, the major axis being supposed to be perpendicular to the axis of the shell. This, of course, is not the tangent cone to the whorls. It follows that if the curves in the plane of the major axis remain regular, and yet one plane can be made to touch the surface at the extremities of all the minor axes, the shell must either be unsymmetrical or the shape of the section must vary; and the latter will certainly be the case when *two* such planes can be found. We cannot, therefore, measure the apical angle of a *Cyrtoceras* or *Dentalium*, in a plane perpendicular to that of the curvature, with perfect accuracy.

17. When a discoid shell has suffered distortion (that is, compression in the plane of the major axes), β may still be found if more than half a whorl be left; but no method sufficiently simple to be of use can give either α or β on distorted fragments. A more important case is when the compression is perpendicular to the plane of the major axis. This has no effect on α, but increases β at the expense of κ. In measuring the apical angle of *Orthocerata*, we therefore require the following:—

Given the semi-vertical angle of a cone of elliptical section when the excentricity of the section is e, *to find it when the same is compressed so that the excentricity is* e′.

Describe a sphere with the vertex as centre; then the cone will cut this sphere in a curve of double curvature, the length of which will remain constant when the excentricity is altered, since the area of such a cone will be unaltered by compression.

If $\pi-2\omega$ be the vertical angle of the cone in the plane of the major axis $2a$, and h the height, then $\frac{h}{a}=\tan\omega$.

Let ϕ be the complement of the excentric angle at any point of the ellipse, whose excentricity is e, and ρ the radius from the centre of the section δ, $d\sigma$ the element of the arc on the sphere, ds of the ellipse, $d\theta$ the angle between ρ and $\rho+\delta\rho$, A the radius of the sphere; then

$$d\sigma=\mathrm{A}d\theta,$$

$$\rho^2d\theta^2=ds^2-d\rho^2,$$

and $$\rho=\sqrt{h^2+a^2(1-\mathrm{e}^2\cos^2\phi)},$$

and $$ds=a\sqrt{1-\mathrm{e}^2\sin^2\phi}\,d\phi;$$

$$\therefore\ d\theta=a\frac{\sqrt{h^2+a^2(1-\mathrm{e}^2)-h^2\mathrm{e}^2\sin^2\phi}}{h^2+a^2(1-\mathrm{e}\cos^2\phi)}d\phi,$$

$$d\sigma=\mathrm{A}\frac{\sqrt{\sec^2\omega-\mathrm{e}^2(1+\tan^2\omega\sin^2\phi)}}{\sec^2\omega-\mathrm{e}^2\cos^2\phi}d\phi,$$

whence the required relation deduced from the equality of the areas is

$$\int_0^{\frac{\pi}{2}} \frac{\sqrt{\sec^2\omega - e^2(1+\tan^2\omega\sin^2\phi)}}{\sec^2\omega - e^2\cos^2\phi}\,d\phi = \int_0^{\frac{\pi}{2}} \frac{\sqrt{\sec^2\omega' - e'^2(1+\tan^2\omega'\sin\phi)}}{\sec^2\omega' - e'^2\cos^2\phi}\,d\phi.$$

The impossibility of exact measurements on fossil shells which have been compressed renders the working out of these integrals, representing the correct solution of the question, a waste of time, if a fair approximation may be made some other way. No very close approximation which is practicable to work has been found. If, for example, we take for the length of the arc the sum of the chords subtending $\frac{1}{4}$ the arc of a circle passing through the ends of the major axis and the spherical projection of the minor axis, and if ω_1, ω_2 be the angles subtended at the centre by the semiaxes, then

$$\kappa = \frac{\tan\omega_2}{\tan\omega_1},$$

and

$$\text{radius of circle} = \frac{A(1-\cos\omega_1\cos\omega_2)}{\sqrt{1-2\cos\omega_1\cos\omega_2+\cos^2\omega_1}},$$

$$\cos\theta = \frac{\cos\omega_1(\cos\omega_2-\cos\omega_1)}{1-\cos\omega_1\cos\omega_2},$$

whence

$$\sin\frac{\theta}{4} = \sqrt{\frac{\sqrt{2(1-\cos\omega_1\cos\omega_2)}-\sin\omega_1}{2\sqrt{2(1-\cos\omega_1\cos\omega_2)}}};$$

and if the chords be assumed constant, we have

$$C = \frac{(1-\cos\omega_1\cos\omega_2)^2}{2(1-\cos\omega_1\cos\omega_2)-\sin^2\omega_1}\cdot\frac{\sqrt{2(1-\cos\omega_1\cos\omega_2)}-\sin\omega_1}{\sqrt{1-\cos\omega_1\cos\omega_2}}$$

or

$$\frac{(1-\cos\omega_1\cos\omega_2)^{\frac{3}{2}}}{\sqrt{2(1-\cos\omega_1\cos\omega_2)}+\sin\omega_1} = \frac{(1-\cos\omega'_1\cos\omega'_2)^{\frac{3}{2}}}{\sqrt{2(1-\cos\omega_1'\cos\omega_2')}+\sin\omega'_1}.$$

If this relation is satisfied between two shells, one may be the compressed form of the other.

For example, a specimen of *Orthoceras annulatus* has its minor axis decrease from 20 to 16 lines in 38, the major axis at the larger size being 32; to compare this with M'Coy's description "tapering 1 in 8," the uncompressed section being

circular. Here

$$\tan \omega_2 = \frac{4}{38} = \frac{2}{19};$$

$$\tan \omega_1 = \frac{8}{5} \times \frac{4}{38} = \frac{16}{95};$$

also

$$\tan \omega'_1 = \tan \omega'_2 = \frac{1}{8}.$$

In this case the two above expressions become respectively ·0071 and ·0048. Hence the tapering in the compressed shell compared is more rapid. If a conical shell be perfectly flattened and its transverse ornaments become circular arcs, the length of any semicircumference $=\pi a$ gives the original diameter of the shell when circular.

18. In the case of a curved shell of elliptical section being compressed perpendicularly to its median plane, we may assume that the area remains constant, and find the effect on the angle β.

If $a = e^{\theta \cot \alpha} \sigma$, the element of area

$$= e^{2\theta \cot \alpha} \sigma \sqrt{1 - e^2 \sin^2 \phi}\, d\phi (1 - \sigma \sin \phi) d\theta$$

when ϕ is measured from the end of the minor axis towards the pole, and the same with $1 + \sigma \sin \phi$ instead of $1 - \sigma \sin \phi$ when measured away from the pole. Integrating with respect to θ between the limits θ and $-\infty$, the area may be written

$$\frac{\sigma e^{2\theta \cot \alpha}}{2 \cot \alpha} \int \sqrt{1 - e^2 \sin^2 \phi} (1 \pm \sigma \sin \phi) d\phi.$$

Now, denoting $\int_0^\phi \sqrt{1 - e^2 \sin^2 \phi}\, d\phi$ by $\mathrm{E}(e\phi)$ as usual, and $\int_0^\phi \sqrt{1 - e^2 \sin^2 \phi} \sin \phi\, d\phi$ by $\mathrm{M}(e\phi)$,

$$\text{area} = \frac{\sigma e^{2\theta \cot \alpha}}{2 \cot \alpha} \left\{ \mathrm{E}\left(e \frac{\pi}{2}\right) + \mathrm{E}(e\phi) + \sigma \mathrm{M}\left(e \frac{\pi}{2}\right) - \sigma \mathrm{M}(e\phi) \right\}.$$

Hence, α remaining unaltered, the condition that a compressed and uncompressed shell may belong to the same species is

$$\sigma \mathrm{E}\left(e \frac{\pi}{2}\right) + \sigma \mathrm{E}(e\phi) + \sigma^2 \mathrm{M}\left(e \frac{\pi}{2}\right) - \sigma^2 (\mathrm{M}e\phi) = \left\{ \sigma' \mathrm{E}\left(e' \frac{\pi}{2}\right) + \sigma' \mathrm{E}(e'\phi') + \sigma'^2 \mathrm{M}\left(e' \frac{\pi}{2}\right) - \sigma'^2 \mathrm{M}(e'\phi') \right\} e^{2 \cot \alpha (\theta' - \theta)}. \quad (1)$$

To find $M(e\phi)$, put $\cos\phi = x$;

$$\therefore \int \sqrt{1-e^2\sin\phi}\sin\phi\, d\phi = -\int \sqrt{1-e^2+e^2x^2}\,dx = -e\int \sqrt{\frac{1-e^2}{e^2}+x^2}\,d$$

$$= -\frac{e}{2}\left\{\frac{x}{e}\sqrt{1-e^2+e^2x^2} + \frac{1-e^2}{e^2}\log\left(x+\frac{1}{e}\sqrt{1-e^2+e^2x^2}\right)\right\}.$$

When

$$\phi = 0, \quad x = 1;$$

$$\therefore M(e\phi) = \frac{e}{2}\left\{\frac{1}{e} + \frac{1-e^2}{e^2}\log\left(1+\frac{1}{e}\right) - \frac{\cos\phi}{e}\sqrt{1-e^2\sin^2\phi}\right.$$

$$\left. - \frac{1-e^2}{e^2}\log\left(\cos\phi + \frac{1}{e}\sqrt{1-e^2\sin^2\phi}\right)\right\}$$

$$= \tfrac{1}{2}.(1-\cos\phi\sqrt{1-e^2\sin^2\phi}) - \frac{1-e^2}{2e}\log\frac{e\cos\phi+\sqrt{1-e^2\sin^2\phi}}{1+e};$$

consequently

$$M\left(e\,\frac{\pi}{2}\right) = \tfrac{1}{2} - \frac{1-e^2}{4e}\log\frac{1-e}{1+e}.$$

It now remains to determine σ and ϕ in terms of R and λ, and θ' in terms of θ. If ϕ, ϕ_1 be the complements of PBN, PAN respectively, we have from the figure (fig. 9),

$$\sigma\sin\phi = (1-\lambda-\lambda\sigma)\left(=\frac{BN}{OB}\right), \quad . \quad . \quad . \quad . \quad (2)$$

$$R^2\sin\phi + \sin\phi_1 = \frac{R^2-1}{\sigma}\left(=\frac{AB}{OB}\right), \quad . \quad . \quad (3)$$

$$R^2\cos\phi = \cos\phi_1\left(=\frac{PN}{OB}\right); \quad . \quad . \quad . \quad . \quad . \quad (4)$$

from these we obtain

$$\sigma = 1 - \frac{2R^2\lambda}{R^2+1}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (5)$$

By the compression of the shell the radii, and therefore the θ's corresponding to them, will be enlarged; and this will take place so that the sum of the arcs of the ellipses in one plane may be constant. This gives

$$\sigma e^{\theta\cot\alpha}\left\{E\left(e\,\frac{\pi}{2}\right) + \frac{R^2E(e\phi)+E(e\phi_1)}{R^2-1}\right\}$$

$$= \sigma' e^{\theta'\cot\alpha}\left\{E\left(e'\frac{\pi}{2}\right) + \frac{R^2E(e'\phi')+E(e'\phi_1)}{R^2-1}\right\}, \quad (6)$$

whence, making the necessary substitutions in (1), we obtain the condition for identity.

When the whorls do not intersect, we must put $\phi=\frac{\pi}{2}$ in (1) and cancel the fractions in (6): the rest are not required. The only important application of these conditions is when a shell has been completely flattened in shaly beds. In any case crumplings of the surface may be induced by resistance to lateral expansion, when the expression for the area of the compressed shell will come out less than it ought; but if it comes out *greater*, the two shells compared could not have had the same original form. An example may be take in *Ammonites planorbis*, which, when uncompressed, has been called *Ammonites erugatus*. In an example of the latter we find R=1·33, λ=·431; whence by (5) σ=·449; and by (2) $\sin\phi$=·838. The last whorl on which these measures were taken was 10·83 millims. in breadth; $\therefore$ 10·833$-a$=·838a, or a=5·894. The thickness of the same whorl was 8·33 millims., whence e=·7065. The elliptic integrals are best observed directly by measurement of the arc; and if the values differ slightly from the theoretical values, the correction has probably to do with the deviation of the form from a perfect ellipse. In this case

$$2a\left\{\mathrm{E}\left(\mathrm{e},\frac{\pi}{2}\right)+\mathrm{E}(\mathrm{e},\phi)\right\}$$

= the circumference of the whorl between its intersections with the inner whorl = 26 millims, whence

$$\mathrm{E}\left(\mathrm{e},\frac{\pi}{2}\right)+\mathrm{E}(\mathrm{e}'\phi)=2\cdot205.$$

Also, on substitution,

$$\mathrm{M}(\mathrm{e},\phi)=\cdot408,\text{ and }\mathrm{M}\left(\mathrm{e},\frac{\pi}{2}\right)=\cdot811.$$

Hence the first side of equation (1) becomes 1·071. In the flattened shell as seen in the type, R=1·33 and λ'=·384, whence σ'=·49 and $\sigma'\sin\phi'$=·428. Also, e′ being unity,

$$\mathrm{E}\left(\mathrm{e}',\frac{\pi}{2}\right)+\mathrm{E}(\mathrm{e}',\phi)=1+\sin\phi',\text{ and }\mathrm{M}(\mathrm{e}'\phi')=\frac{\sin^2\phi'}{2},$$

and

$$\mathrm{M}\left(\mathrm{e}',\frac{\pi}{2}\right)=\tfrac{1}{2}.$$

Hence the second side of the equation reduces to

$$1\cdot038\,.\,e^{2(\theta'-\theta)\cot\alpha}.$$

Now we have seen that

$$\mathrm{E}\left(\mathrm{e},\frac{\pi}{2}\right)+\mathrm{E}(\mathrm{e},\phi)=2\cdot205;$$

and by direct measurement we find that

$$\mathrm{E}(e, \phi) + \mathrm{E}(e, \phi_{,}) = 1{\cdot}037\,;$$

whence, from (6),

$$e^{2(\theta' - \theta)\cot\alpha} = 1{\cdot}139$$

(the coefficient on the right-hand side becoming $1 + \sigma'$). The second side of (1) becomes now 1·184. This value does not agree sufficiently with 1·071 for the difference to be due to any errors of observation; and it follows that *Ammonites planorbis* was produced from a more involute shell than that called *Ammonites erugatus*: and such more involute varieties occur in the south of England.

19. *To find the angle of elevation in a turbinated shell.*

The point in the tracing-curve whose angle of elevation is measured must, of course, depend on the shape of that curve. When it is such as to form a complete cone, as in *Eulima*, the angle for any point on the surface is directly measurable. When the curve is such as the ellipse, the angle measured by the tangent lines does not correspond to the γ already used, but is connected with it by an easily expressed relation; for if ω be the semi-vertical angle of the tangent cone, the equation (A) must give equal roots when $r\cot\omega$ is written for z, which requires that

$$\tan\gamma = \cot\omega + \sigma\sqrt{\kappa^2(\cot\omega\cot\epsilon + 1)^2 + (\cot\omega - \cot\epsilon)^2}.$$

When, as is often the case, $\omega = \epsilon$, this reduces to

$$\tan\gamma = \frac{\cos\omega + \sigma\kappa}{\sin\omega}.$$

It is obvious that when ω and γ can both be observed directly, this gives an equation to find σ, as noted in § 15.

An example of the use of this equation may be taken from the previously quoted *Cyclostoma elegans*. Here

$$\sigma = {\cdot}46, \quad \omega = 24^\circ, \quad \epsilon = 156^\circ, \quad \kappa = {\cdot}87315,$$

whence

$$\tan\gamma = 4{\cdot}874 = \tan 78^\circ\,24',$$

the angle derived from direct measurement being $78\frac{1}{2}^\circ$.

20. The semi-vertical angle of the cone for any particular point may also be calculated by means of other angles often more easily measured. Let ω be such an angle, and i the angle between the tangent at that point and the axis. Let AB ($\delta\sigma$) be an element of the curve at that point, DB its projection on the plane of xy (δs). Then

$$dz = d\sigma\cos i, \quad ds = d\sigma\sin i, \quad dz = dr\cot\omega, \quad dr = ds\cos\alpha\,;$$

whence

$$\tan \omega = \cos \alpha \tan i. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

If χ be the angle between the tangent and the radius from the vertex,

$$dz \sec \omega = d\sigma \cos \chi;$$

$$\therefore \cos \omega = \cos i \sec \chi. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

The values, therefore, of i and χ are sufficient to give ω and α. This last equation was obtained by Professor Moseley by a different method, and the first, under a different form, deduced from it. It follows from (1) that i increases with ω in the same shell, and hence that the angles must be measured at corresponding points. In the same way χ is dependent on ω, since from (1) and (2) we may deduce

$$\tan \chi = \tan \alpha \sin \omega. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

The angle χ is the only one which can be well measured on specimens, and i only on figures. The "sutural angle" employed by D'Orbigny is different from either of these, being the angle between the line joining the ends of two radii corresponding to θ and $\theta + \pi$ with the smaller of them. The connexion of this with the drawing of the suture is only approximate, as the latter does not lie in one plane. If ψ be this angle, we have

$$e^{\pi \cot \alpha} = \frac{\sin \psi}{\sin (\psi + 2\omega)},$$

or

$$\tan \psi = \frac{\mathrm{R} \sin 2\omega}{1 - \mathrm{R} \cos 2\omega}. \quad . \quad . \quad . \quad . \quad . \quad (4)$$

If ω be found from the tangent lines to the shell, ψ must be measured from the points of contact and not in the suture—whence the term "sutural" angle is inexact for a second reason. This method has the advantage, however, of being applicable to very little more than a complete whorl, the rest requiring one and a half. When less than one whorl is preserved, it is sometimes possible to measure the angle between the projections of two tangents to the curve at points separated by half a whorl, on a plane through the axis perpendicular to that containing them, *i. e.* between BG (fig. 10) and the corresponding line on the opposite side. If 2ϕ be this angle, we have

$$\tan \phi = \frac{dz}{r d\theta} = \cot \omega \tan \alpha.$$

One important value of these equations is to serve as a check on the separate measurements, and thus to gain obser-

vations for drawing an average, as there is no doubt that spiral shells are not absolutely geometrically constant, and all measures are therefore more or less approximate.

One example of their use will suffice: this may be *Phasianella striata*. In this we have $\omega = 16°$; i by observation is 79°. Also R = 1·2; to this value corresponds in the Table 86° 41′. Substituting these values of ω and α in (1) we obtain $i = 78° 41'$. Again, substituting in (4) for R and ω, we obtain $\psi = 91° 36'$; by observation the same angle is 92°. These results are sufficiently close to prove the regularity of the shell formation in this case.

It has not been thought worth while to study the effects of compression on turbinated shells, because from their shape no assumption can be made as to the direction of pressure that could be considered generally applicable.

XXXIII. *On the Limits of Hypotheses regarding the Properties of the Matter composing the Interior of the Earth. By* Henry Hennessy, *F.R.S., Professor of Applied Mathematics in the Royal College of Science for Ireland**.

1. From direct observation we are able to obtain only a very moderate knowledge of the materials existing below the solid crust of the earth. The depth to which we can penetrate by mining and boring operations into this crust is comparatively insignificant; and these operations give us little knowledge of the earth's interior in comparison with what is afforded by the outpourings of volcanos. Two hundred active volcanos are said to still exist, while geologists have established that many thousands of such deep apertures in the earth's crust have existed during remote epochs of its physical history. The source or sources of supply for all these volcanoes have poured out a predominating mass of matter in a state of liquidity from fusion. Evidence is thus furnished that matter in a state of fluidity exists very widely distributed through the earth. The supposition that this fluid fills the whole interior, and that the solid crust is a mere exterior envelope, is usually designated as the hypothesis of internal fluidity. From this hypothesis mechanical and physical results of primary importance in terrestrial physics may be deduced.

Newton, Clairaut, Laplace, Airy, and other illustrious mathematicians have used an extension of this hypothesis in discussing the earth's figure. They supposed the particles com-

* Communicated by the Author, having been read before the Mathematical and Physical Section of the British Association for the Advancement of Science, Dublin, August 1878.

posing the earth to retain the same positions after solidification as that which they held before it. I ventured, for the first time, to discard the latter portion of the hypothesis as useless and contrary to physical laws. I now venture to say that, in framing any hypotheses as to the physical character of the matter of the earth, we should not affix any property to the supposed matter which is opposed to the properties observed in similar kinds of matter coming under our direct observation. Observation has disclosed that liquids are in general viscid, and that they possess what has been designated internal friction in a high degree*. Observation has recently shown that among the three states of matter (gaseous, liquid, and solid) a law of continuity exists. Observation also discloses that gases and vapours are, of all forms of matter, the most compressible, that liquids are much less compressible, and that solids are still less compressible. Thus, for instance, water is about fourteen times more compressible than copper or brass.

2. If these general comparative properties of liquids and solids are admitted, it follows that in the hypotheses regarding the earth's internal structure we should most carefully guard against any assumption directly in contradiction to such properties. By assuming that the earth contained a fluid totally devoid of viscidity and internal friction, the late Mr. Hopkins attempted to prove the earth's entire solidity. He only proved that it did not contain any of this imaginary fluid; but he by no means proved the non-existence of a liquid possessing the properties of viscidity and internal friction common to all liquids. In the *Comptes Rendus* of the Academy of Sciences of Paris for 1871 is a paper in which I have given a *résumé* of the arguments against Mr. Hopkins's conclusions as to the earth's complete solidity; and in the subsequent discussions my priority on this matter seems to have been fairly and honourably acknowledged†. In a recent admirable work on Geology, Pfaff's *Grundriss der Geologie*, the author gives a brief account of the bearing of astronomical and mathematical investigations on the internal structure of the earth; and he very justly says that the results of observation compel us to regard the earth as for the most part fluid, in order to bring these results into harmony with calculation. Professor Pfaff

* As having a special connexion with this subject, see a Report by the Author on Experiments on the influence of the molecular condition of fluids on their motion when in rotation and in contact with solids (Proceedings of the Royal Irish Academy, 2nd series, vol. iii. p. 55).

† "Remarques à propos d'une Communication de M. Delaunay sur les résultats fournis par l'Astronomie concernant l'épaisseur de la croûte solide du Globe," *Comptes Rendus de l'Inst. France*, Mars 6, 1871, p. 250.

attributes this conclusion to Hopkins, whereas it is precisely that which I had long since enunciated, and is entirely opposed to the views of Mr. Hopkins. More recently Sir William Thomson and Mr. Darwin have investigated the tidal action of an internal fluid nucleus upon its containing solid shell. They have both supposed the liquid to be totally incompressible, and the containing vessel to be elastic and therefore compressible. They have thus given the liquid a property which no liquid in existence possesses, and the solid a property which solids possess in a much less degree than liquids. Their hypothesis is thus totally inadmissible as a part of the problem of inquiry into the earth's structure. I at once admit that a thin elastic spheroidal envelope filled with incompressible liquid and subjected to the attractions of exterior bodies would present periodical deformations, owing to tidal action far surpassing the tides of the ocean. But I do not admit that such impossible substances can represent the materials of the earth. My hypothesis is that the liquid interior matter, instead of being incompressible, is, like all liquids we observe, relatively far more compressible than its solid envelope. A highly compressible liquid contained in a very much less-compressible shell would be a hypothesis more in harmony with physical observation. The tidal phenomena of a compressible fluid, it is easy to see, would be very different from those of an incompressible fluid. The work done by the action of certain disturbing bodies in the strata of compressible fluid would partly result in causing variations of density, instead of producing tidal waves of great magnitude. This has been already shown in the *Mécanique Céleste* by Laplace, in discussing the tides of the atmosphere. Theory shows that the atmospheric tides should be nearly insensible, notwithstanding the great depth of the atmospheric column, because the work done in the atmosphere is very different from what is peformed in the less-compressible water of the ocean. Observation has fully verified this result.

3. It is admitted that the earth's density increases from its surface towards its centre. If its interior is occupied by a compressible fluid, the law of density of this fluid would result from the compression of its own strata; just as the law of density of the atmosphere is produced by the pressure of the upper atmospheric layers upon those below. But instead of supposing the interior of the earth to be filled by a fluid thus conforming to the observed properties of fluids, both Sir William Thomson and Mr. Darwin have applied their great powers as accomplished mathematicians to the tides of an incompressible and homogeneous spheroid, such as I admit to have no real existence whatsoever.

4. The labour bestowed on the problem investigated could scarcely be considered at all necessary or fruitful, except as affording an admirable illustration of the results flowing from the employment of hypotheses framed in direct contradiction to the fundamental conditions to which every truly philosophical hypothesis must conform. It is scarcely necessary to add, that the conclusions of Mr. Darwin, as well as those of Sir William Thomson, cannot be considered as having invalidated the carefully framed hypothesis that the earth consists of a solid crust physically similar to the rocks we are enabled to observe, and a contained spheroid of liquid matter partaking of the established properties of liquids, and physically similar to the liquid rock poured out by volcanic openings.

5. It is with much satisfaction that I can trace a gradual growth of more correct physical views on the questions referred to in this paper. In 'Nature,' vol. v. p. 288, a paper appeared in which I ventured to criticise Sir William Thomson's memoir on the Rigidity of the Earth, in the 'Philosophical Transactions.' At the Meeting of the British Association in Glasgow, Sir William Thomson acknowledged the invalidity of many of his arguments, and requested his audience to draw their pens through paragraphs from 23 to 31 in his paper. These paragraphs contain statements and reasonings which I had already shown to be inconclusive in the paper which has just been quoted.

In Mr. Darwin's paper, recently communicated to the British Association, he admits that in discussing the precessional and tidal phenomena of a viscous liquid, the supposition of an elastic spheroid would lead to *very different* results—that is to say, results very different from those deduced by himself and Sir William Thomson regarding the earth's structure, and which the followers of the late Sir Charles Lyell have frequently assumed to be established. Thus the late Mr. Poulett Scrope appears to have referred to the bearing of the mathematical investigations alluded to, on what he calls "the sensational idea" of an internal incandescent fluid beneath the solid crust of the earth. He forgot that an idea may not be the less true because it is sensational. The idea of antipodes was at one time regarded as highly sensational. Those who witness a great earthquake or a volcanic eruption are usually impressed with the sensational character of the phenomena.

6. A traveller who was in Portugal more than forty years since, met a woman over one hundred years of age, and asked her if she recollected the great earthquake of Lisbon. She replied, that it was the event of all others in her long life which she ought to vividly recollect, on account of its impres-

sive sensations. History also records the sensational character of the destruction of Pompeii. If Mr. Scrope's innuendo regarding the internal fluidity of the earth as "a sensational hypothesis" has any value, we should regard the events referred to as highly improbable; yet they have been as well authenticated as the most positive facts in science, and no person has ever expressed the smallest shadow of a doubt as to their occurrence.

XXXIV. *On the Blue Colour of the Sky.* *By* Arthur Mason Worthington, *Trin. Coll. Oxford, M.A., F.R.A.S., Head Master of the Salt Schools, Shipley.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

In the series of articles which Mr. Norman Lockyer is publishing in the columns of 'Nature,' entitled "Physical Science for Artists," he broaches an explanation of certain effects of aerial perspective and sky-colour, which appears to me inconsistent in itself. It is, moreover, entirely at variance with the explanation which I believe to be generally accepted by those who have thought about the matter, and which is stated without any reserve by Professor Helmholtz in the last volume of his popular lectures published in 1876, in a lecture entitled "Optisches über Malerei." Mr. Lockyer writes with well-deserved authority; and it is, I think, for this very reason unfortunate that he should set before professedly unscientific readers (since he writes for artists) a novel explanation of his own, without at least indicating that there is an entirely different and generally accepted explanation of the phenomena of which he is speaking; for he alludes (p. 156, § 4) to Dr. Tyndall's conclusion as *somewhat similar* to his own.

I wish specially to draw attention to the apparent inconsistency of his explanation of the blue colour of the air so frequently seen between the observer and a distant mountain.

In No. 4 of the series (May 30) he explains that gold is yellow because, out of the white light which enters it, the components at each end of the spectrum are absorbed in their transmission into the mass of metal, and so there remain over only the middle rays, which, if transmitted, appear green, if reflected, yellow, owing to the greater quantity of the light. Clearly he regards the colour of gold as green (or yellow), because the *other* colours are absorbed. He then quotes from Professor Stokes's admirable South-Kensington Lecture, and accepts his conclusion, that a poppy is red because out of the

white light that enters it and emerges from it to be reflected, only the red survives absorption ; the rest is absorbed in the interior of the leaf.

So far, beyond his own explanation of the molecular groupings which cause this selective absorption, Mr. Lockyer has advanced nothing new ; he has dealt only with what are called "natural bodies." But in No. 6 of June 6th, he apparently applies the explanation of the colours of natural bodies to the colour of the air between the observer and a distant hill ; and here I cannot follow him.

He says, page 155, § 2 :—

"We are in the presence of aqueous vapour competent to be set in vibration by blue light, and because it vibrates in this way it appears blue." And in the next paragraph:—

"If the stratum of aqueous vapour had had a background of bright sky, it would have absorbed the blue light of that sky. By virtue of the principles which I have stated, the sky would have appeared red in consequence of the abstraction of blue light."

Mr. Lockyer here clearly regards the molecules as abstracting the blue rays ; as being set in synchronous vibration by them, and therefore sending blue light to the eye. He can hardly mean that the phenomenon is one of selective absorption and emission, for no vapour emits visible light until its temperature is very high : indeed there seems no doubt that he considers the air to appear blue by *reflected* light ; for he says, speaking of similar water-vapour, on page 156, §§ 2 and 3:—"Let us consider, then, the action of those molecules which absorb the blue light.

"Now, since these molecules absorb blue light, we know that they will reflect blue light, and, practically speaking, nothing else. Here, then, we have the cause for the blue colour of the sky."

Why ! Mr. Lockyer has himself explained that gold is yellow because it absorbs the red and blue and reflects the yellow light ; that a poppy is red because it absorbs all but the red ; and now he says that water-vapour is blue because it *absorbs* blue.

Apart from the inconsistency of this explanation, I do not think that, as a matter of fact, Professor Stokes's explanation of the colours of "natural bodies" can be applied to the appearance of a mass of air in which particles of aqueous vapour are suspended. The following is what Professor Helmholtz says of the same phenomenon, translated from the lecture I have mentioned. On page 65, § 3, he says:—"Under the head of aerial perspective we understand the optical effect of the

appearance of light proceeding from the illuminated mass of air lying between the spectator and a distant object. This appearance of light is due to a slight turbidity, from which the atmosphere is never quite free. Whenever there are scattered through a transparent medium small transparent particles whose density and ability to deviate the light are different from those of the medium, these particles turn aside out of its direct course any light which falls upon them in traversing the medium, and, partly by reflection and partly by refraction, 'disperse' it in all directions, to use an expression of optics." After mentioning, as sources of turbidity in the air, dust, smoke, organic matter, water-vapour on the point of condensation, and the presence of hot and cold currents of air, he goes on to say that, as regards the cause of turbidity in the higher and drier regions of the atmosphere, which produces the blue light of the sky, whether we have to do here with particles of foreign matter, or whether the molecules of the air itself act as a turbidity in the æther, science has no certain information to give.

"As regards, however, the colour of the light reflected by the disturbing particles," he resumes, "this depends practically on their size. When a log of wood floats on water, and we excite small wave rings by letting a drop of water fall near it, these waves are reflected by the floating log as though it were a fixed wall. But in the long waves of the sea, such a log of wood would be tossed up and down without the form of the waves being materially altered in their progress. Now light also is known to be an undulatory motion transmitted through the æther that fills space. The red and yellow rays have the longest waves, the violet and blue the shortest. Very small bodies disturbing the continuity of the æther will therefore reflect the latter rays notably more than the former. In fact, the finer the disturbing particles are, the bluer the light of the turbid medium; while larger particles reflect more equally light of every colour, and for this reason cause a whiter turbidity. Of such kind is the blue of the sky, *i. e.* of the turbid atmosphere seen against black space. The purer and more transparent the air, the bluer the sky. In like manner it becomes bluer and darker when we ascend a high mountain, partly because the air high up is freer from turbidity, partly because there is, of course, less air above our head. But the same blue which we see before the dark background of space, appears also before terrestrial objects, *e. g.* distant mountains in shadow or wooded, whenever a thick layer of illuminated air lies between them and us. It is the same 'air-light' which makes both sky and mountain blue; only in the former

case it is pure ; in the latter it is mixed with other light proceeding from the objects behind, and moreover belongs to the coarser turbidity of the lower layers of the atmosphere, for which reason it is whiter. In the drier air of warmer countries the turbidity is finer, even in the lower strata of the atmosphere, and hence the blue seen before distant terrestrial objects is more like that of the sky. It is to this circumstance that Italian landscapes owe their clearness and richness of colour." I fail to detect in this explanation, which was originally, I believe, due to Dr. Tyndall, the slightest resemblance to that of Mr. Lockyer.

I am, your obedient servant,

A. M. Worthington.

XXXV. *Note on Acoustic Repulsion.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

PERHAPS the following explanation of the curious phenomenon of the repulsion of resonators observed by Dvořák and Mayer* may be of interest to the readers of the Philosophical Magazine.

The hydrodynamical equation of pressure for irrotational motion is (in the usual notation)

$$\varpi = \int \frac{dp}{\rho} = \mathrm{R} - \frac{d\phi}{dt} - \tfrac{1}{2}\mathrm{U}^2. \quad . \quad . \quad . \quad (1)$$

If we suppose that there are no impressed forces, R=0. Distinguishing the values of the quantities at two points of space by suffixes, we may write

$$\varpi_1 - \varpi_0 = \frac{d}{dt}(\phi_0 - \phi_1) - \tfrac{1}{2}\mathrm{U}_0^2 + \tfrac{1}{2}\mathrm{U}_1^2. \quad . \quad . \quad . \quad (2)$$

This equation holds good at every instant. Integrating it over a long range of time, we obtain as applicable to every case of fluid-motion in which the flow between the two points does not continually increase

$$\int \varpi_1 dt - \int \varpi_0 dt = \tfrac{1}{2}\int \mathrm{U}_1^2 dt - \tfrac{1}{2}\int \mathrm{U}_0^2 dt. \quad . \quad . \quad . \quad (3)$$

Let us now apply this equation to the case of a resonator excited by a source of sound nearly in unison with itself, taking the first point at a distance from the resonator, where neither the variation of pressure nor the velocity is sensible, and for the second a point in the interior of the cavity, where

* Phil. Mag. September 1878, p. 225.

the velocity is negligible, but, on the other hand, the variation of pressure considerable. It follows that

$$\int(\varpi_1-\varpi_0)dt=0, \quad . \quad . \quad . \quad . \quad . \quad . \quad (4)$$

or that the mean value of ϖ in the interior is the same as at a distance outside.

The remainder of the investigation depends upon the relation between p and ρ. If the expansions and contractions are isothermal, $p=a^2\rho$, and $\varpi=a^2\log p$. Thus

$$\int\log p_1 dt=\log p_0 . t; \quad . \quad . \quad . \quad . \quad . \quad (5)$$

or the mean logarithmic pressure in the interior is the same as the constant logarithmic pressure at a distance. Equation (5) may also be written

$$\int\log\left(1+\frac{p_1-p_0}{p_0}\right)dt=0, \quad . \quad . \quad . \quad . \quad (6)$$

or

$$\int\left\{\frac{p_1-p_0}{p_0}-\tfrac{1}{2}\left(\frac{p_1-p_0}{p_0}\right)^2+\ldots\right\}dt=0; \quad . \quad . \quad (7)$$

whence, if the changes of pressure be relatively small, we see that the mean value of p_1-p_0 is positive, or, in other words, that the mean pressure inside the resonator is in *excess* of the atmospheric pressure.

If, as in practice, the expansions and contractions are adiabatic, $p\propto\rho^\gamma$, where $\gamma=1{\cdot}4$, and (5) is replaced by

$$\int p_1^{\frac{\gamma-1}{\gamma}}dt=p_0^{\frac{\gamma-1}{\gamma}}.t. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (8)$$

Thus, instead of (7),

$$\int\left\{\left(1+\frac{p_1-p_0}{p_0}\right)^{\frac{\gamma-1}{\gamma}}-1\right\}dt=0; \quad . \quad . \quad . \quad (9)$$

whence, by the binomial theorem,

$$\int\frac{p_1-p_0}{p_0}dt=\frac{1}{2\gamma}\int\left(\frac{p_1-p_0}{p_0}\right)^2dt \quad . \quad . \quad . \quad . \quad (10)$$

approximately, showing that here again the mean pressure in the interior of the cavity exceeds the atmospheric pressure.

Hence, on either supposition, the resonator tends to move as if impelled by a force acting normally over the area of its aperture and directed *inwards*.

I am, Gentlemen,

Your obedient Servant,

RAYLEIGH.

Terling Place, Witham,
September 7, 1878.

XXXVI. *On certain Phenomena accompanying Rainbows.* By Silvanus P. Thompson, *D.Sc., B.A., F.R.A.S., Professor of Experimental Physics in University College, Bristol**.

The literature of the Rainbow is somewhat extensive; yet I do not recollect having anywhere seen any record of certain phenomena, of radial streaks of light, which are occasionally to be seen accompanying rainbows. I am convinced that the phenomenon is not rare; and in the desire of inducing more observers to watch for its occurrence, I beg to present the few notes which follow. On the evening of July 8th, 1877, I stood with a friend upon the summit of the Drachenfels about an hour before sunset. To the south and south-east the sky was obscured by dark masses of cloud; and the sky above us and to the north and west was covered with broken clouds: there was no perceptible wind. A fine rainbow spanned the valley, and extended eastwards toward the Löwenberg. Both primary and secondary bows were fine; and several supernumeraries were noticed. As the bow faded away a faint streak of light was observed, not unlike the streak of an aurora, outside the right limb of the secondary bow, extending up to it, about 12° of arc long by 2° wide, and making an angle of about 15° with the horizon. It lasted fully ten minutes, maintaining nearly same position and form. It was perfectly free from colour. A similar, fainter streak was observed on the left limb at about 70° to the horizon. Standing upon the very summit of the Drachenfels, the profile of the hill and our shadows were rudely projected upon the nearest hillside; it was very easy, therefore, to verify that these streaks were really radial in direction. Each of them, if produced backwards, would have passed through the point of space exactly opposite the sun.

July 15th, 1877, after a very wet and stormy day, we observed, from the promenade in front of the Schweitzerhof at Lucerne, a very fine rainbow at sunset. The colours were unusually brilliant. Seven supernumeraries of the primary bow were distinctly visible, and one of the secondary. The clouds behind presented a uniform deep grey tint. A colourless streak was visible within the primary bow, radial in position and at about 45° to the left.

July 16th, 1877, about 6.30 P.M., from the high road near Sarnen, the same observers watched the same phenomenon. The day was thundery and with frequent showers and gusts from the north. A very fine and complete bow was seen,

* Communicated by the Author, having been read before Section A of the British Association, Aug. 19th, 1878.

partly against the sky, partly against the hills which bound on the south-east the high road from Alpnach. At intervals several radial streaks were observed both interior and exterior to the rainbow. They shifted position and magnitude rapidly. It was noticeable that to each streak corresponded a patch of sunshine on the hills behind, each streak pointing to a patch and moving with it.

Three times in June 1878 I observed at Bristol similar phenomena accompanying rainbows, though not so favourably.

The explanation which is suggested by the observation made at Sarnen is very simple. These wedge-shaped radial streaks are "beams" of sunlight, and become visible by diffuse reflexion from particles of matter in their path, just as the apparently divergent beams of sunrise or sunset become visible. These "beams" being practically parallel to one another, appear to converge in the point exactly opposite the sun by perspective; or, in fact, just as the parallel beams of sunset appear divergent. Since the rainbow has for its centre the point opposite the sun, such beams must necessarily have positions radial with respect to the bow. They resemble, therefore, the "*rayons du crépuscule*" occasionally seen in the east at sunset, or in the west at sunrise.

Here let me mention one peculiarity of the radial streaks, in which they appear to differ from the "rayons du crépuscule." I have never observed a "streak" crossing the dusky region between the primary and secondary bows, though I have seen one and the same streak extend beyond the bows both outside the secondary and inside the primary. I have never seen a "streak" of colourless light cross the coloured part of a rainbow. I have noticed a *faint* bow crossed by a streak; where the streak crossed it, the bows (primary, secondary, and supernumerary) became more vivid. There was more light; but it was dispersed in the usual way, and the intermediate region between the bows was no brighter where the streak crossed it. I conclude, then, that such a streak is the very stuff, so to speak, of which rainbows are made; only when the neighbouring regions are obscured by clouds, the beam of light which struggles through builds up its own portion of the arch in its appropriate place.

Two not dissimilar phenomena (quite independent of the rainbow) appear to confirm this conclusion. Stand in the sunlight, when the sun is high, so that the shadow of your head falls upon the surface of a slightly turbid pond or lake, whose surface is covered with gentle waves, or ripples: you will see the shadow of your head surrounded by a halo of quivering radial streaks of light. From the deck of a steamer on the

Thames I have often observed this. The waves or ripples of the surface cause the sun's rays to enter the water with unequal intensities at different points. At certain points the beams will enter almost without loss by reflexion, and will be traceable by their illuminating the particles of the turbid water. These beams will be nearly parallel to one another; but as they all retreat from the observer, they will appear to converge to a point exactly opposite the sun; to a point within the shadow of his head, in fact, giving the nimbus effect.

The other analogous phenomenon requires to be explained by reflexion. If you stand upon a ridge, or a high wall in the sunlight, so that your shadow falls upon a field of waving corn, you will notice that the corn-field appears to be illuminated in the region all round the shadow of your head. This is best seen when travelling by railway, with the sun about 50° above the horizon, from the top of an embankment, so that the shadow of the train and observer fall upon the corn-fields below.

University College, Bristol.
July 31, 1878.

XXXVII. *On the principal Screws of Inertia of a Free or Constrained Rigid Body.* *By* Robert S. Ball, *LL.D., F.R.S., Royal Astronomer of Ireland**.

IN the following paper I propose to treat of the effect of an impulse upon a quiescent rigid body, so far as the initial movement of the body is concerned. The analytical investigation of this problem is so well known, that I do not propose to enter into that subject at present. I believe, however, that the ordinary method of viewing the question may be supplemented by the purely geometrical or physical treatment which I shall endeavour to sketch. This geometrical aspect of the problem, as it has presented itself to my mind, has been developed in the course of certain researches in the dynamics of a rigid body which I have ventured to call the Theory of Screws. I shall here indicate the more salient points of this theory which are necessary for the present question.

The most important feature of the geometrical method of viewing the subject is its extreme naturalness, as well as the wide generality with which the problem is grasped. If the rigid body were perfectly free, the questions presented are comparatively simple, so much so that there is not a great deal of interest attached to the investigation. But when we consider the case of a rigid body whose movements are more or

* Communicated by the Author, having been read before the British Association, Dublin, August 1878.

less constrained, the problems possess a high degree of geometrical interest. It is also the occasion of some surprise that, notwithstanding the infinite variety of conceivable constraints (such as fixed points, axes, contact with fixed surfaces, arrangements of link-work and the like) by which the movements of the rigid body may be hampered, their geometrical classification is of the utmost simplicity. It will be shown that there are six fundamental descriptions of constraint, which include every conceivable arrangement, from leaving the body absolutely free on the one hand, or absolutely immovable on the other. With the investigation of these fundamental forms of constraint we may fitly commence our inquiry.

In the first place, it is to be observed that, as the constraints limit the movements of a body, we may adequately describe the nature of those constraints either by pointing out all the movements of the rigid body which are *prohibited*, or, on the other hand, by ascertaining all the movements which are *permitted*. It is obviously more to the purpose to adopt the latter method of viewing the subject; and therefore we shall proceed to indicate the method by which a complete inventory may be made of the possible movements which a rigid body can execute. *It is also to be continually borne in mind that we are only considering the initial movements of the body, and that, consequently, it is only necessary to consider movements which are of indefinitely small magnitude.*

Let it therefore be supposed that the rigid body is submitted to our examination when it occupies a definite position A. We are not now going to apply the impulsive forces to it; we are at present merely making a preliminary trial of a purely geometrical or kinematical character of its capability for displacement. It is at once perceived that the body, not being fixed, can be moved into many closely adjacent positions. Take any one of these positions and call it B.

By a celebrated theorem of Chasles it is known that the displacement of the body from A to B can be produced by translating the body parallel to a certain line, and at the same time rotating the body about the same line. We may, for convenience, speak of this motion as a *twist*; and we may term the angle of rotation the *amplitude of the twist*. The distance of the translation is proportional to the amplitude of the twist, and may be taken to be equal to the product of the amplitude of the twist and a certain linear magnitude called the *pitch*. The axis about which the rotation is made, associated with the linear magnitude termed the pitch, constitute what is called a *screw*. We therefore say that the displacement of the rigid body from A to B is effected by a twist about a screw.

It is to be observed that the twist involves two elements, *i. e.* a graphic element (the screw), and a metric element (the amplitude of the twist). A little reflection will show that the constraints which permit displacement from A to B must also admit of *any* infinitely small twist being made upon the screw defined by A and B. We may therefore say that the body is free to twist about the screw (A, B). If we find on examition that the body cannot be displaced to any position *except* those which could be attained by twists of suitable amplitude about the screw (A, B), then we can assert that the body has *only one degree of freedom*; and that freedom is perfectly expressed by the capacity to twist on one definite screw.

It is easy to verify in particular cases the general principle that, no matter what the constraints be, a body which has but one degree of freedom can twist about a certain screw and can have no other movements. For example, a body free to rotate about an axis, but not to slide along it, can only twist about a screw of which the pitch is zero, or a body free to slide along an axis, but not to rotate around it, can only twist about a screw whose pitch is infinite. A less obvious instance is presented in the case of a body of which five points are limited each to a given surface; but even in this case the body is still only free to twist about a certain screw. Draw the five normals to the surfaces, and regard them as the rays of a linear complex. Then the screw about which the body can twist is the principal axis of that complex, while the pitch of the screw is its parameter. These, however, are only illustrations; our concern is with the general proportion that *when a body has but one degree of freedom, from whatever cause arising, it is free to twist about one screw, and only one.*

Suppose, however, it were found that the body, besides being able to twist about a certain screw α, was also able to twist about a second screw β, then twisting about an infinite number of other screws must also be possible. For the position attained by a twist about α, followed by a twist about β, could have been reached by a single twist about some screw γ. Now as the amplitude of the twists about α and β are arbitrary, it is obvious that γ must be one of a singly infinite number of screws which include α and β. It follows that the body must be able to twist about all the screws which constitute the generators of a certain ruled surface.

This surface is called the *cylindroid*: it is of the third order, its equation being

$$z(x^2+y^2)-2\,mxy=0.$$

The cylindroid is already well known to the students of the linear geometry of Plücker.

A body which has two degrees of freedom is therefore able to twist about all the screws which lie upon a cylindroid. This is true, no matter what be the nature of the constraints. It is a matter worthy of notice that notwithstanding the infinite variety of constraints which would permit a body to have two degrees of freedom, that freedom must still be completely defined by a cylindroid, although all cylindroids are similar surfaces, and possess no variety except as to absolute size. It should also be remarked that the pitches of the screws on the cylindroid are proportional to the inverse squares of the parallel diameters of a certain conic.

If it be found that a body can be twisted about three screws which do not lie upon the same cylindroid, then the body must be capable of being twisted about a doubly infinite number of screws. Of this doubly infinite number three pass through each point in space, all the screws of given pitch lie upon a hyperboloid, and all the screws parallel to a plane lie upon a cylindroid. The pitch of each screw of the system is proportional to the inverse square of the parallel diameter of a certain quadric called the *pitch-quadric*; and the pitch-quadric is itself the locus of the screws of zero pitch. The entire system is determined when the pitch-quadric is known. These kinematical theorems are intimately connected with Plücker's geometrical speculations on a system of three linear complexes.

Included in the case of freedom of the third order, we have the celebrated problem where a body is rotating around a point. In this case, however, the pitch-quadric assumes an evanescent form, and the general conception of the capabilities of a body which has freedom of the third order are very much degraded.

If a body be able to twist about four screws which do not all belong to such a system as that we have just been describing, then the body must be able to twist about a trebly infinite number of screws. A cone of screws of this system passes through each point of space; and the cone may be drawn by letting fall perpendiculars from the point upon the generators of a cylindroid. It is remarkable that these cones are of the second order; and it can also be shown that the feet of their perpendiculars upon the generators of the cylindroid are in the same plane. It is thus to be observed that the capabilities of motion possessed by a body which has freedom of the fourth order are completely determined when a certain cylindroid is given in size and position; for then all the cones are determined.

In the case where a body can twist about five screws not belonging to a system such as that we have just been

describing, then the system of screws is quadruply infinite; in fact, the body can then twist about one screw of a certain pitch on every line in space.

Finally, if the body can twist about six screws not belonging to the system just mentioned, then the body has freedom of the sixth order, and is, in fact, perfectly free.

We thus see that, corresponding to each order of freedom, a certain group of screws is appropriate; and we may call such a group a *screw-system* for the sake of brevity. Thus, in the case of freedom of the second order the screw-system is a cylindroid; in the case of freedom of the fourth order the screw-system consists of all the screws of proper pitch which intersect a generator of a cylindroid at right angles, and so on.

By this preliminary investigation we are enabled to dismiss entirely all further mention of the constraints. Every conceivable form of constraints can only give the body permission to twist about one of the six types of screw-system. I have not in this brief summary attempted to give any demonstrations of the different theorems involved. For these, reference may be made to the 'Theory of Screws'*.

We have now laid the foundation of the first part of the problem to be discussed in this paper, inasmuch as we have shown how the body *may* move; the next question is to ascertain how the body *will* move when it receives an impulse.

It will, however, be first necessary to consider the most general form of impulse which the body can receive. Now it is well known that all the forces acting upon a rigid body may be reduced to a single force, and a couple in a plane perpendicular to that force. The efficiency of the couple is expressed by its moment; and the moment is the product of a force and a linear magnitude. It will not be unnatural to associate this force and couple with the conception of the screw, already introduced. We may use the expression *wrench* to denote a force along a screw and a couple in a plane perpendicular to the screw, the moment of the couple being equal to the product of the force and the pitch of the screw. Thus, every system of force acting upon a rigid body constitutes a *wrench upon a screw*; and it is completely determined when the screw on the one hand, and the force on the other, are both given.

The analogy subsisting between the twist and the wrench, as implied by their mutual connexion with the abstract geometrical conception of a screw, will be the main source of the theorems now to be enunciated.

The original problem has now been brought into this condition. On the one hand, we have a body whose freedom is

* Ball's 'Theory of Screws:' Dublin, 1876.

expressed by a certain screw-system ; and, on the other hand, we have a group of impulsive forces which constitute a wrench on what may be called the *impulsive screw*. Now, in consequence of this impulse the body will commence to move ; but the only motion it can execute must be to twist about some screw of its screw-system ; there must therefore be in the screw-system a certain *instantaneous screw* corresponding to each *impulsive screw*. If the impulsive screw be given, the instantaneous screw is determined ; but the converse is not true. In fact, the instantaneous screw is limited to the screw-system, whereas the impulsive screw may be anywhere in the universe and with any pitch. It is, however, possible to clear away this ambiguity in a very satisfactory manner. It can be shown that, whenever the body is not quite free, there are a group of impulsive screws corresponding to each instantaneous screw. It is not here necessary for us to enter into the properties of these groups of impulsive screws, further than to remark that, if a body receive an impulsive wrench upon any one of the screws belonging to this group, it will commence to twist about the instantaneous screw to which the group is correlated. In each group of impulsive screws there is one screw which merits most particular attention. It can be proved that in each of these groups there is always one screw (but only one) which not only belongs to the group but also belongs to the screw-system expressing the freedom of the body. It therefore follows that the effect of an impulsive wrench anywhere in space could have been produced by an impulsive wrench on a screw suitably chosen from among the members of the screw-system itself. We need therefore only consider the effect of impulsive wrenches upon screws which actually belong to the screw-system.

In this way the ambiguity has been dissipated without any sacrifice of generality : to each screw of the system regarded as an instantaneous screw corresponds another screw of the complex regarded as an impulsive screw, and *vice versâ*.

The study of this system of correspondence between the impulsive screw and the instantaneous screw of the same screw-complex, leads to many results of considerable interest, not only on account of their great generality, but also on account of their geometrical character. It is natural to consider, in the first place, whether there are any common elements in the two corresponding systems ; and we are thus led to the discovery that *when a rigid body has freedom of the* n*th order, then* n *screws can be selected from the screw-system expressing that freedom such that, if the body receive an impulsive wrench on any one of these screws, the body will commence to twist about the same screw*. These are called the *principal screws of inertia*.

We shall illustrate the existence of these principal screws of inertia by pointing them out in the particular cases of freedom of the second and third orders. When the body has freedom of the second order, the screw-system is a cylindroid. All the generators of a cylindroid are parallel to a plane ; and by the anharmonic ratio of four generators is to be understood the anharmonic ratio of four parallel rays drawn through any point. Now it can be shown that the anharmonic ratio of four instantaneous screws on the cylindroid is equal to the anharmonic ratio of the four corresponding impulsive screws. When, therefore, three impulsive screws and the three corresponding instantaneous screws are known, the instantaneous screw corresponding to any impulsive screw is at once determined by geometry. The double rays of the two equianharmonic systems must of course be parallel to the two principal screws of inertia on the cylindroid ; and thus the problem of finding the principal screws of inertia for freedom of the second order is completely solved.

In the case of freedom of the third order, the three principal screws of inertia can also be completely determined by geometry. For this purpose it is necessary to construct a certain ellipsoid, which is defined by the following theorem.

The kinetic energy of a rigid body when twisting with a given twist velocity about any screw of a screw-system of the third order is proportional to the inverse square of the parallel diameter of a certain ellipsoid.

If this ellipsoid be made concentric with the pitch-quadric, it will be possible to draw a triad of common conjugate diameters to the two surfaces ; and the required principal screws of inertia are the three screws of the complex which are parallel to these conjugate diameters.

In that special case of freedom of the third order in which a body is rotating about a fixed point, then the general property of the three principal screws of inertia degrades to the well-known property of the principal axes. It will be observed that the theory here propounded may be considered to generalize the property of the principal axes into a general property for freedom of the third order, and then further into a property for freedom of any order.

We conclude by pointing out the six principal screws of inertia of a perfectly free rigid body. They are found as follows :—Draw the three principal axes, A, B, C, through the centre of gravity, and let a, b, c be the radii of gyration; then two screws on A with the pitches $+a$, $-a$, and two similar screws on B and on C, constitute the six principal screws of inertia.

Dunsink, Co. Dublin, August 1878.

XXXVIII. *On the Discharge of Water from Orifices at different Temperatures.* *By* Professor W. C. Unwin, *M.I.C.E.**

In the Journal of the Franklin Institute for May 1878, there is a paper by Chief-Engineer Isherwood, of the U.S. Navy, giving an account of some experiments on the discharge of water from orifices at different temperatures. Those experiments appear to have been made on a sufficient scale and with very great care; and they lead to the conclusion that temperature has a very marked influence on the discharge. The author evidently supposes his conclusions to be applicable to orifices in general; for he remarks that, "in the various determinations which have been made of the ratio of the actual to the theoretical discharge of water through orifices, the temperature of the water should have been noted. The experimental ratios are true for only the experimental temperatures, and need reduction to a standard temperature."

The experiments were made by noting the time in which the level of the water in a cylindrical vessel fell from one level to another, the water being discharged from a given orifice. The observed results of the experiments are not given. These results were plotted in a diagram, and a fair curve drawn passing as evenly as possible through the plotted points. The results are stated to have been corrected for the dilatation of the orifice by heat; but it is not stated whether any correction was made for the dilatation of the volume of the vessel from which the water flowed, a correction quite as important as the other. The following short Table gives a few of Mr. Isherwood's results, as measured by him from the curve representing the observed results:—

Temperature, Fahrenheit.	Relative time of discharge for equal volumes.	Relative velocities of discharge for equal volumes.
32°	1·0000	1·0000
60	0·9896	1·0105
100	0·9696	1·0313
140	0·9457	1·0577
180	0·9156	1·0922
212	0·8855	1·1293

Thus the velocity of discharge increases 12 per cent. as the temperature rises from 32° to 212°, and it increases 8 per cent. as the temperature rises from 60° to 180°.

Now there is this difficulty in accepting the results of Mr. Isherwood's experiments,—that the actual velocity of discharge at ordinary temperatures differs from the whole velocity due to the head by only from 3 to 6 per cent. for simple orifices.

* Communicated by the Physical Society.

It is not easy to see that any increase of fluidity of the water or diminution of friction could do more than annul this loss of from 3 to 6 per cent. Mr. Isherwood's experiments seem to imply that the velocity of discharge at high temperatures may be greater than the velocity due to the head. It seemed worth while therefore to repeat the experiment. The means at the author's disposal did not permit him to make the experiments on quite so large a scale as those of Mr. Isherwood; but it was, he believes, quite large enough to indicate any gain of velocity of the amount mentioned above.

A cast-iron cistern was used, the interior bored out to a diameter of 0·4 metre. The first orifice tried was a carefully formed brass conoidal orifice, formed as nearly as might be to the shape of the vena contracta, and very approximately 0·01 metre diameter. Three pointed indexes were fixed in the cistern, below the surface of the water; so that, as the level of the water descended, the instant at which the point broke the surface could be very exactly observed. Calling the levels of the three indexes A, B, and C, the time was noted in which the water-level descended from A to B and from A to C. A chronograph-watch was used, the seconds' hand being started at the moment the water-level was at A. When the level reached B, the time elapsed was noted by an observer counting seconds. When the level reached C the hand of the watch was stopped. This last observation was perhaps more reliable than the intermediate one at the level B.

For the higher temperatures the water was taken from a steam-engine boiler; it was somewhat discoloured by iron and sediment. The same water was used in the experiments at lower temperatures, but in its ordinary clean condition. The author does not think that the condition of the water made any sensible difference in the results of the experiments.

Experiments on a Conoidal Orifice, June 4, 1878.

Temperature, Fahrenheit.	Time of fall of water-level in seconds.	
	From A to B.	From A to C.
190°	59	89
190	58½	88½
130	59½	90
60	62	93
60	61½	92½
	Mean Results.	
190	58·75	88·75
130	59·5	90·00
60	61·75	92·50

These results show a distinct increase of velocity of discharge at the higher temperatures; but the increase from 60° to 190° is only 4 per cent., or less than half the increase observed by Mr. Isherwood.

When water issues from an orifice with gradually diminishing head, the relation between the time and volume of discharge is given by the well-known equation

$$t = \frac{\Omega}{c\omega}\left\{\sqrt{\frac{2h_1}{g}} - \sqrt{\frac{2h_2}{g}}\right\}, \quad . \quad . \quad . \quad . \quad (1)$$

where Ω = area of water-surface in reservoir,
ω = area of orifice,
h_1, h_2 = heads at beginning and end of experiment above centre of orifice,
t = time of outflow in seconds,
c = coefficient of discharge.

Let D = diameter of the cylindrical reservoir, and d = diameter of the orifice. Then

$$\frac{\Omega}{\omega} = \frac{D^2}{d^2}.$$

Hence the equation above becomes

$$t = \frac{D^2}{cd^2}\sqrt{\frac{2}{g}}\{\sqrt{h_1} - \sqrt{h_2}\}.$$

In these experiments D = 15·7 inches, d = 0·3937 inch. Reducing these values to feet and introducing them in the equation,

$$t = \frac{394{\cdot}376}{c}\{\sqrt{h_1} - \sqrt{h_2}\}.$$

The heights of the index-points above the centre of the orifice were, as nearly as could be measured,

A	1·467 foot.
B	1·133 „
C	0·9775 „

Hence, for experiments in which the level fell from A to B,

$$c = \frac{57{\cdot}895}{t}; \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

and for experiments in which the level fell from A to C,

$$c = \frac{87{\cdot}748}{t}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

Neglecting for the present the expansion of the reservoir and orifice, the coefficients of discharge deduced from the experiments are as follows:—

Coefficients of Discharge.

	Fall of level from A to B.	Fall of level from A to C.
190°	·9813 ·9896	·9859 ·9915
130°	·9730	·9750
60°	·9338 ·9414	·9435 ·9486

Mean Values of c.

190°	0·9871
130°	0·9740
60°	0·9418

The experiments on a conoidal orifice having shown a small but definite influence of temperature on the discharge, it seemed desirable to try whether a similar effect would be produced in the case of a thin-edged orifice. With the conoidal orifice there is no contraction of the jet, and the discharge is less than the so-called theoretical discharge by an amount depending only on the friction of the orifice. In the case of a thin-edged orifice, the jet contracts to an area of about $\frac{5}{8}$ that of the orifice; and the discharge is diminished not only by the friction, but also to a much greater extent by the contraction. The thin-edged orifice was 1 centimetre diameter; and the heads were nearly the same as before.

Experiments on Thin-edged Orifice, 1 centim. in diameter, June 12, 1878.

Temperature, Fahrenheit.	Time of discharge of water between A and B in fifths of a second.	Time of discharge of water between A and C in fifths of a second.
205°	503	748
205	500	747
140	507	744
65	500	746
61	495	740
61	495	740

Mean Values.

62°	496·6	742
140	507·0	744
205	501·5	747·5

The times here recorded show that, in the case of a thin-edged orifice, the temperature has an extremely small influence

on the discharge, and that, unless the small differences in the experiments are errors of observation, the discharge is greater at low temperatures. Whether this is due to the increase of temperature increasing the contraction at the same time as it diminishes the friction, could only be determined by much more extensive experiments.

For these experiments, when the level fell from A to B,

$$c = \frac{292{\cdot}75}{t}; \quad \ldots\ldots\ldots \quad (4)$$

and when the level fell from A to C,

$$c = \frac{443{\cdot}71}{t}, \quad \ldots\ldots\ldots \quad (5)$$

t being in fifths of a second.

Coefficients of Discharge for Thin-edged Orifice.

Temperature, Fahrenheit.	Values of c. Fall of level from A to B.	Fall of level from A to C.
205°	·5820	·5932
205	·5855	·5940
140	·5774	·5964
65	·5855	·5948
61	·5914	·5996
61	·5914	·5996
Mean Values.		
205°	·5837	·5936
140	·5774	·5964
62	·5894	·5980

These results seem to show that the temperature has hardly any sensible influence on the discharge from orifices of this kind.

It will be seen that the results of these experiments do not at all agree with those of Mr. Isherwood; and although made on a smaller scale, the author believes that if the influence of temperature had been nearly as great as that stated by Mr. Isherwood, it could not possibly have escaped detection. Minute errors in measuring the head or the size of the orifice would sensibly affect the values of the coefficients obtained; and these may possibly be wrong to the extent of 2 or 3 per cent.; but these errors would not affect the relative values of the coefficients in any sensible degree, and the author therefore

believes that temperature has a far less influence on the discharge from simple orifices than Mr. Isherwood's results would imply. It is difficult to explain to what the higher results obtained by Mr. Isherwood are due; but the conjecture may be hazarded that the orifice in his experiments was very exceptionally placed. It was at the end of a bell-mouthed tube some 10 inches long, a great part of which was only $\frac{3}{4}$ inch diameter; and there was a plug-cock immediately above the orifice. It seems possible that there was a good deal of friction in this pipe, and that the diminution of friction in this part of the apparatus led to the increase of discharge as the temperature increased *.

Thus far the effect of the temperature on the capacity of the reservoir and the size of the orifice has been neglected. It only remains to examine whether the expansion of these has any material influence on the results.

The effect of temperature on the quantities entering into the equation of flow is twofold. First, the ratio $\frac{D^2}{d^2}$ is altered, because the mouthpiece was of brass and the reservoir of cast iron; and the former expands more than the latter. Secondly, the level marks being attached to the side of the cistern, the distance between these marks and the centre of the orifice increases as the temperature rises. There is, however, an uncertainty in applying a correction for the expansion of the metal, because, its external surface being exposed to the air, its mean temperature would be less than the temperature of the water. The following estimate of the correction is therefore approximate only.

Let e_b be the expansion of brass per unit of length and per degree;
e_c the expansion of cast iron estimated in the same way;
τ = the excess of temperature during the experiment reckoned from 60°.

* It is impossible to calculate, except roughly, the frictional resistance of the tube to which, in Mr. Isherwood's experiments, the orifices were attached. Taking 4 inches length of pipe $\frac{3}{4}$ inch in diameter, and neglecting the bell-mouthed part, we get, using D'Arcy's coefficient of friction, and putting the data in feet:—

For a discharge of	0·0167	.....	0·0374
„ velocity	5·5	.....	12·2
„ head lost in pipe	0·6	.....	0·12
„ total head	5·0	.....	1·0

So that apparently about 12 per cent. of the head may have been lost in the friction of the pipe leading to the orifice.

Then, in consequence of the expansion of the metal, the ratio of the areas $\frac{D^2}{d^2}$ becomes

$$\frac{(1+e_c\tau)^2}{(1+e_b\tau)^2}\frac{D^2}{d^2},$$

and the true difference of the square roots of the heads is

$$\sqrt{1+e_c\tau}(\sqrt{h_1-h_2}).$$

The formula of flow, allowing for the alteration of the dimensions by rise of temperature, is therefore

$$t=\frac{(1+e_c\tau)^2}{(1+e_b)\tau^2}\sqrt{1+e_c\tau}\,\frac{D^2}{cd^2}\{\sqrt{h_1}-\sqrt{h_2}\}.$$

Let $e_c=\cdot000006$,

$e_b=\cdot00001$,

$\tau=190°-60°=130$;

$$\frac{(1+e_c\tau)^2}{(1+e_b\tau)^2}\sqrt{1+e_c\tau}=\left(\frac{1\cdot00078}{1\cdot0013}\right)^2\sqrt{1\cdot00078}=0\cdot999355.$$

Hence it is obvious that the effect of the expansion of the reservoir and orifice is very small for the range of temperature in these experiments. Allowing for that expansion, we get for the experiments at 190°,

$$c=\frac{\cdot99935\,D^2}{td^2}\{\sqrt{h_1}-\sqrt{h_2}\},$$

or slightly smaller values of the coefficient than those given above.

It is rather curious that it is stated in Mr. Isherwood's paper that the results are corrected for the variation of the size of the orifice as the temperature varied, but no mention is made of a correction for the size of the reservoir or the expansion of the vessel to which the index-marks denoting the initial and final heads were attached. If these latter corrections have been omitted, though this is difficult to believe, Mr. Isherwood's results should be divided by

$$(1+e_c\tau)^2\sqrt{1+e_c\tau},$$

where e_c is the coefficient of expansion of the material of the reservoir, whatever that was. This would sensibly diminish the apparent increase of discharge at high temperatures given in his experiments.

XXXIX. *Action of Alkaline Solution of Permanganate of Potash on certain Gases.* By J. A. Wanklyn and W. J. Cooper*.

In continuation of our work on the oxidizing-power of strongly alkaline solution of permanganate of potash, we have made experiments on the common gases, and have arrived at results of some interest. The solution which we have employed in these experiments contained 16 grms. of permanganate of potash and 5 grms. of caustic potash dissolved in a litre of distilled water.

Binoxide of Nitrogen, NO.

This gas was prepared in the usual manner by the action of diluted nitric acid on metallic copper. On submitting it to the above-described solution of alkaline permanganate there was immediate action, the gas being instantly absorbed at ordinary temperatures, and the solution being instantly decolorized and caused to deposit the brown hydrated binoxide of manganese.

110 cubic centims. of NO and 30 cubic centims. of the potash-and-permanganate solution were shaken up together. Immediately the solution lost its colour and deposited the brown binoxide of manganese, and 85 cubic centims. of the gas was absorbed.

The reaction is

$$NO + KMnO_4 = MnO_2 + KNO_3.$$

Protoxide of Nitrogen, N_2O.

This gas, prepared in the usual way from nitrate of ammonia, appears to be quite without action on the alkaline solution of permanganate of potash. Even on prolonged heating of the materials in the water-bath there was no sign whatever of action, the permanganate preserving its brilliancy, and the volume of the enclosed gas undergoing no diminution.

Nitrogen Gas.

Experiments published some years ago by one of us show that this gas is not attacked by the alkaline solution of permanganate, even when the temperature is considerably raised.

Carbonic Oxide, CO.

This gas, prepared by the action of excess of sulphuric acid on ferrocyanide of potassium, is attacked by the alkaline solu-

* Communicated by the Authors.

tion of permanganate. The action is not instantaneous, as in the case of the binoxide of nitrogen, but is comparatively slow.

118 cubic centims. of CO and 30 cubic centims. of the above solution of potash and permanganate of potash were sealed up and heated in the water-bath, being frequently taken out of the bath, cooled, and shaken. Altogether the heating occupied some three or four hours. On opening the tube under water it was found that great absorption of gas had taken place.

Of the 118 cubic centims. of CO taken for experiment,

92 cubic centims. were absorbed,
26 „ „ residue.
118

The action appears to be

$$CO+O=CO_2,$$

regard being had to the amount of $KMnO_4$ which had been reduced during the operation. At ordinary temperatures the action takes place, but very slowly.

Hydrogen

is also absorbed by the alkaline solution of permanganate. In an experiment in which 64 cubic centims. of hydrogen were sealed up with 16 cubic centims. of alkaline permanganate and heated for some hours in the water-bath, an absorption of 34 cubic centims. of hydrogen was noted.

We are continuing the investigation.

XL. *Researches on Unipolar Induction, Atmospheric Electricity, and the Aurora Borealis. By* E. Edlund, *Professor of Physics at the Swedish Royal Academy of Sciences*.*

[Plate VIII.]

"Res ardua rebus vetustis novitatem dare, novis auctoritatem, obscuris lucem, dubiis fidem, omnibus vero naturam et naturæ suæ omnia."—Pliny, *Hist. Nat.* t. i., præfatio.

§ 1. *Unipolar Induction.*

We represent to ourselves a steel magnet in a vertical position, readily set in rotation about its geometric axis; and we picture it besides surrounded by a metallic muff in the form of a cylinder, which can likewise turn about the same axis. If the current of a pile be caused to pass into this

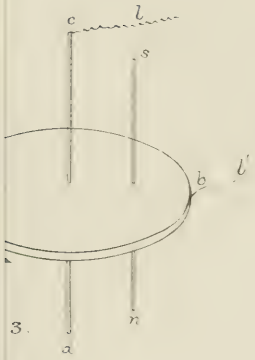
* Translated from a copy, communicated by the Author, of the *Kongl. Svenska Vetenkaps-Akademiens Handlingar*, vol. xvi. No. 1.

cylinder in such manner that one of the electrodes is in contact with it in the vicinity of the poles of the magnet, and the second electrode at a point situated between the two poles, experience shows that the cylinder begins to rotate about the magnet. The direction of rotation depends on that of the current in the cylinder, and also on the situation of the poles. As to the magnet itself, it remains perfectly motionless; consequently the galvanic current does not exert upon it any rotatory action. It is therefore possible to turn the magnet mechanically round its axis without the slightest obstacle being offered by the reciprocal action of the magnet and current; the sole resistance to be surmounted in the mechanical rotation of the magnet is occasioned by the friction in the sockets of the axis, &c., a resistance which has nothing to do with the current. In a previous memoir* I have demonstrated that, according to the mechanical theory of heat, every phenomenon of the sort mentioned will be accompanied by a phenomenon of unipolar induction. In fact, if the pile be removed and replaced by a galvanometer inserted between the two abovementioned electrodes, and in contact with the cylinder, the galvanometer indicates the rise of a current as soon as the cylinder is mechanically put in rotation. The electromotive force here consists of the mechanical work necessary to overcome the reaction of the magnet upon the current in that part of the circuit which is set rotating. This species of induction has received the name of "unipolar induction." The current produced is proportional to the velocity of the cylinder. Of course the simultaneous mechanical rotation of the *magnet* produces no augmentation in the current generated by the rotation of the *cylinder*, since this augmentation would be made without the consumption of a corresponding amount of mechanical work—which would be perfectly absurd. Plücker has also proved by experiment that in this case the rotation of the magnet is incapable of producing a current. Here is another reason:—It has been demonstrated by experiment that, in the case in question, for the magnet a solenoid can be substituted, producing the same effect†. I have myself shown, on a previous occasion‡, that the *rotation* of the solenoid about its axis cannot produce a unipolar induction current, whether proceeding from a single fluid or from two fluids in translatory motion. It is therefore, in this case, the rotation of the *cylinder* about the magnet that gives rise to the observed uni-

* *Öfversigt af kongl. Vetensk.-Akad. Vörh.* 1877; Wiedemann's *Annalen*, vol. ii. p. 347.

† *Öfversigt*, April 1877; Pogg. *Ann.* vol. clx. p. 604.

‡ *Öfversigt*, Jan. 1877; Pogg. *Ann.* vol. clx. p. 617.



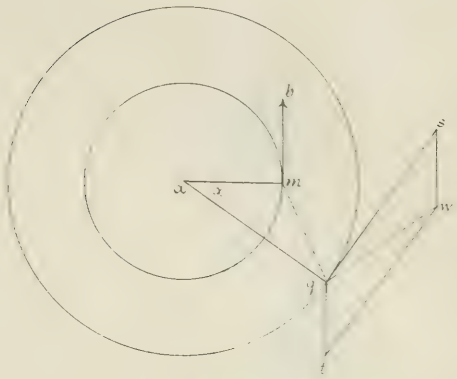


Fig 4

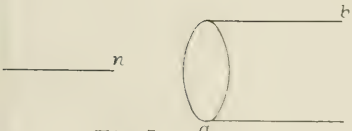


Fig. 7

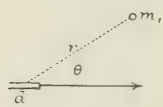
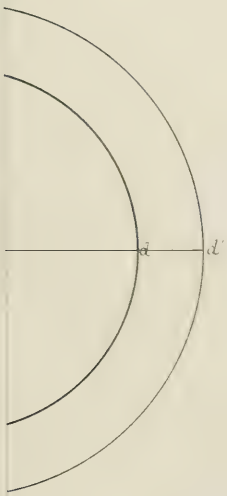


Fig. 8



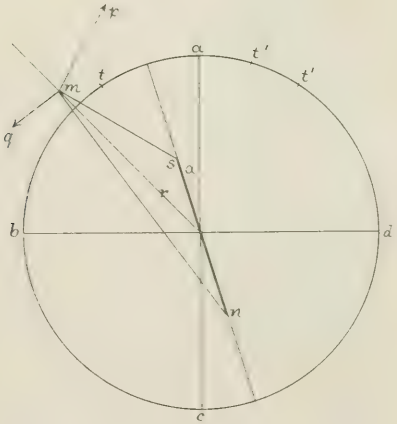


Fig 11

Mintern Bros lith

polar induced current; while the rotation of the *magnet* about its axis has nothing to do with the phenomenon, as several physicists have assumed. If the magnet and cylinder be connected together so as to form a compact body, the magnet is carried in the direction of the rotation of the cylinder without any modification in the induction of the magnet resulting, as we have seen. The magnet acts in this case on the cylinder as if the former were immovable and the latter alone rotating. The opinion expressed by several physicists, that the magnet cannot produce unipolar induction in a conductor with which it is intimately united, is therefore not correct. If the radius of the cylinder be sufficiently reduced for the cylinder to be in perfect contact with the magnet, this circumstance will not prevent induction taking place in the cylinder; and since induction will be produced perfectly irrespective of the thickness of the cylinder, the latter can be entirely removed and the electrodes of the galvanometer put into immediate contact with the magnet without causing the induction to cease. The magnet itself then performs the functions of a conductor, and the induction does not result from its being put in rotation as a magnet, but from its rotation as a conductor. We are here in presence of the phenomenon of unipolar induction first produced by W. Weber*.

If now we are asked how it is that a current is possible in this case, we can reply that this must necessarily take place if our idea of the nature of the galvanic current is correct—namely, that it consists in the translatory motion of a fluid going in the positive direction†, or of two fluids following opposite directions. To understand the necessity of the production of a current in the case in question, one can adopt either of the above-mentioned opinions. In the memoir cited (p. 3), I have recalled the known fact that a metal ring surrounding a magnet and traversed by a galvanic current transports itself, if movable, along the magnet. It stops in the middle, where it takes up a position of stable equilibrium if its galvanic current follows the same direction as the molecular currents of which we imagine the magnet to be formed. If, on the contrary, the current of the ring follows the opposite direction, the ring will be in unstable equilibrium at the middle of the magnet; and if removed from that position, it will continue moving further from the centre until it passes beyond the poles of the magnet. This motion of the ring is determined by the law which regulates the action of a magnetic pole upon

* Pogg. *Ann.* vol. lii. p. 353.

† "Théorie des Phénomènes Electriques," *Kongl. Svenska Akad. Handlingar*, vol. xii.

an element of current, which law, as is known, is formulated as follows:—A single magnetic pole acts on a current-element ds with a force directly proportional to the product of the magnetic moment M of the pole, the intensity i of the current, and the sine of the angle λ between the current-element and the straight line joining the pole of the magnet to the same element, and inversely proportional to the square of the distance r between these last. The force in question may therefore be expressed by $\frac{\mathrm{M}i \sin \lambda \, ds}{r^2}$. The point of application of the force is situated in the current-element; and its direction is perpendicular to the plane which passes through the magnetic pole and the current-element. The sense in which the force acts upon the said direction depends, moreover, on the direction of the current and the nature of the pole.

When the cylinder is put in rotation about the fixed magnet, the electric fluid or the two fluids (if we admit two) put themselves in rotation in the same direction. They form, then, currents with a horizontal circulation round the vertical magnet; and their intensity will be proportional to the velocity of the rotation.

The sole difference between these currents and ordinary galvanic currents is, that the two fluids (if they are two in number) follow the same direction, while in ordinary galvanic currents they take opposite directions. But, according to the usual ideas, the action of the magnet upon the negative current is the same as its action upon a positive current going in the opposite direction. Now the poles of the magnet act, in virtue of the law above cited, upon the currents produced by the rotation of the cylinder; and the consequence of this will be that the cylinder will receive an excess of æther (electropositive fluid) at its two extremities, and a deficiency of æther (electronegative fluid) in the centre, or *vice versâ*, according to the direction of the rotation and the position of the poles. In a conducting wire, one of the extremities of which is in contact with the centre, and the other with one of the edges of the cylinder, there will necessarily arise a galvanic current. The direction of the current thus produced and the augmentation of its intensity with that of the velocity of rotation are perfectly like what takes place in experiment.

In opposition to this view the following objection may now be raised:—If the rise of the unipolar induced current depends on the electric molecules commencing to move round the magnet at the same time as the conductor in which they are present, a similar induced current should also arise if, the conductor and the electric molecules remaining at rest, the mag-

net is put in motion round them, seeing that the phenomenon can only depend on the relative motion between the magnet and those molecules. If, then, it is proved by the above-mentioned experiments that the magnet acts on an electric molecule in rotation about it, it must equally act on the molecule if the latter is at rest while the magnet moves round it. This certainly ought to be the case, and we shall see examples of it in the sequel.

Let us imagine the cylinder divided into vertical columns, each presenting a section equal to unity. To determine quantitatively the force of induction produced in the cylinder by the magnet, it will be sufficient to take one of the columns into consideration. Let us represent the magnet by *ab* (Plate VIII. fig. 1), and name one of the columns *dc*, the distance of which from the magnet shall be indicated by r. Let s designate the south pole, and n the north pole, and $2l$ the distance from the one to the other. Suppose that the cylinder, viewed from above, revolves round the magnet in the opposite direction to that of the hands of a watch. The excess of æther (electropositive fluid) will then collect at the extremities of the column, and the deficiency will make itself sensible in the centre. If the angular velocity be designated by v, the velocity of the column will be equal to rv. The intensity of a current can be expressed by qav, in which q denotes a constant, a the section of the conductor, and v the velocity of the electric fluid. The intensity of the current produced by the rotation in any element dz can then be designated by $qrvdz$, in which q is a constant, and rv, as we have seen, the velocity of the before-mentioned element. If straight lines be drawn from the two poles to the element dz situated in k at the distance z from the line *fe*, and if *kg* and *kh* are perpendicular to the lines mentioned, the south pole will drive the æther (electropositive fluid) along *kg*, while the north pole will lead it along *kh*. Designating by M the intensity of the magnetic poles, the first force will be represented by

$$\frac{+\mathrm{M}qrvdz}{(l-z)^2+r^2},$$

and the second by

$$\frac{\mathrm{M}qrvdz}{(l+z)^2+r^2}.$$

The component of these forces along the column *cd* will then be

$$\frac{+\mathrm{M}qr^2vdz}{[(l-z)^2+r^2]^{\frac{3}{2}}}-\frac{\mathrm{M}qr^2vdz}{[(l+z)^2+r^2]^{\frac{3}{2}}}.$$

Supposing the length of the cylinder equal to the distance between the poles of the magnet, or $2l$, by integration we shall obtain the force conducting the electric fluid of the cylinder to its extremities, namely

$$2\mathrm{M}qvl\left[\frac{1}{(l^2+r^2)^{\frac{1}{2}}}-\frac{1}{(4l^2+r^2)^{\frac{1}{2}}}\right]. \quad . \quad . \quad . \quad (\mathrm{A})$$

This last expression constitutes the electromotive force produced.

We will now see if this expression of the electromotive force conforms to the exigences of the mechanical theory of heat. If a current equal to unity be made to pass through the column from f to d, the column begins to rotate in the opposite direction to that in which we considered it to move by the action of the external mechanical force. From the law previously given, it is easy to calculate the force with which the magnet acts upon the current. The squares of the distances of the two poles from the element dz, situated at the distance z from the line fe, are $(l-z)^2+r^2$ and $(l+z)^2+r^2$; and $\sin\lambda$ is equal, in the two cases, to $\dfrac{r}{[(l-z)^2+r^2]^{\frac{1}{2}}}$ and $\dfrac{r}{[(l+z)^2+r^2]^{\frac{1}{2}}}$ respectively. The force with which the magnet acts upon the current in the direction normal to the plane containing the poles of the magnet and the element dz is therefore

$$\frac{\mathrm{M}r\,dz}{[(l-z)^2+r^2]^{\frac{3}{2}}}-\frac{\mathrm{M}r\,dz}{[(l+z)^2+r^2]^{\frac{3}{2}}},$$

the integral of which, between the limits indicated, is

$$\frac{2\mathrm{M}l}{r}\left[\frac{1}{(l^2+r^2)^{\frac{1}{2}}}-\frac{1}{(4l^2+r^2)_{\frac{1}{2}}}\right]. \quad . \quad . \quad . \quad . \quad (\mathrm{B})$$

It has been demonstrated in my memoir* that, according to the mechanical theory of heat, the electromotive force of induction resulting from the rotation of the cylinder with a velocity $w=rv$ will be equal to a constant (the value of which depends on the unit chosen to designate the intensity of the current) multiplied by the product of the expression (B) and rv. In fact, by multiplying rv into that expression we find again the previously given expression (A).

We obtain, then, the following result:—If it be admitted that, in the case in question, unipolar induction is produced by the action of the magnet on the currents due to the electric molecules being carried along in the direction of the rotation of the cylinder, we shall get for the electromotive force an ex-

* Wiedemann's *Annalen*, vol. ii. p. 347.

pression conformable to the requirements of the mechanical theory of heat.

We will now treat another case of induction. *sn* (fig. 2) represents a powerful magnet, the south pole of which is in *s*, and the north pole in *n*; and *abcd* is a circular metal plate having its centre in the prolongation of the geometrical axis of the magnet, and its plane perpendicular to the same axis. If the plate be set in rotation about the axis *so*, the electric fluid will be carried in the direction of the rotation, and the velocity of an electric molecule will be proportional to its distance from the centre *o*. Now let us imagine a plane containing the axis of the magnet and an electric element *dz* situated in *m*, at the distance *r* from the centre of the plate. Then let the right lines *mq* and *mt* be drawn in this plane, respectively perpendicular to the lines *sm* and *nm*. The line *mq* is then perpendicular to the plane which passes through the south pole and the tangent of the orbit of the element *dz*; and the line *mt* is perpendicular to the plane passing through the same tangent and the north pole. The plate being in rotation in the direction indicated by the arrows, the electric element is urged by the south pole towards *q*, and by the north pole towards *t*. Let *l* be the distance between the poles, *p* the distance from the north pole to the centre of the plate, *r* the distance from this last to the point *m*, and M the force of the poles; further, let *k* be a constant, and *v* the velocity of rotation of the plate: the force with which the element *dz* is urged by the south pole towards the periphery of the plate (or, which comes to the same, the component of the action of the same pole upon *dz* along the plane of the plate) will be given by

$$\frac{k\mathrm{M}vr(l+p)dz}{[(l+p)^2+r^2]^{\frac{3}{2}}},$$

and the force with which the north pole tends to direct the same element towards its centre will be expressed by

$$\frac{k\mathrm{M}vrpdz}{(p^2+x^2)^{\frac{3}{2}}}.$$

By equating these two expressions we shall obtain the value of *r* for which the two forces make equilibrium. We get this value from the equation

$$r^2=(l+p)^{\frac{4}{3}}p^{\frac{2}{3}}+(l+p)^{\frac{2}{3}}p^{\frac{4}{3}}.$$

The circle whose radius has this value may be called the neuter circle. If, for example, we take $l=10$ and $p=5$ centims., we get $r=12{\cdot}7$ centims. The electric molecules situated between the neuter circle and the periphery of the plate are directed by

the magnetic force towards that periphery; and those which lie between that circle and the centre are driven towards the latter. As the formula shows, the value of r of the neuter circle increases with l and p. If the plate be touched outside of the neuter circle with the ends of the electrodes of a galvanometer, the galvanometer indicates a current which goes from the point of contact situated nearest the periphery, through the galvanometer, to the other point of contact. If, on the contrary, both points of contact lie between the neuter circle and the centre of the plate, the current passes from the point of contact situated nearest the centre, through the galvanometer, to the other point of contact. If the neuter circle lies between the two points of contact, the positions of these latter can be selected so as to give rise to no current. If the plate moves in the reverse direction, the electric fluid accumulates around the neuter circle, and the current consequently changes its direction. These deductions from theory have been verified by the observations of M. Felici*.

Plücker has experimentally investigated the following case:—Through the centre of a metal disk, *ab* (fig. 3), passes an axis, *cd*, of the same metal, about which the disk can be set in rotation. Nearer to the circumference of the disk an aperture is pierced, into which a magnet, *sn*, is fitted so that its centre is in the plane of the disk, and its axis parallel to the axis of rotation. If the rotation takes place in the direction indicated by the arrow, the poles of the magnet having the position shown in the figure, on uniting by a conducting wire one of the extremities of the axis of rotation to the circumference of the disk we obtain a galvanic current passing from the former to the latter through the wire. The current consequently follows the same direction as if the disk with its axis were in rotation about the magnet†.

According to what precedes, the induction at any point of the conductor depends on its motion relatively to the magnet regarded as a fixed point. As is known, this relative motion undergoes no modification if to the magnet as well as to the point in question equal velocities in the same direction be imparted. Thus, in the first place, it is easy to determine the induction in the axis of rotation itself by proceeding as follows:—To the rotation-axis and the magnet a velocity is imparted equal but opposite to the actual velocity of the magnet, which therefore comes to rest, while the axis rotates about it in the direction indicated by the arrow. The induction in the axis consequently becomes the same as if the magnet were at

* *Ann. de Chimie et de Physique*, (3) xliv. p. 343.

† Pogg. *Ann.* vol. lxxxvii. p. 352.

rest and the axis were revolving round it with the same angular velocity. As regards the induction in the metallic disk, it can be determined in the following manner:—

Let a (fig. 4) be the point of the disk in which the axis is fixed, m that in which the disk is traversed by the magnet, and p the distance between these points. The rotation-velocity may then be expressed by pv, v denoting the angular velocity, and this being effected in the direction from m to b. Any point, q, of the disk, at the distance r from the axis, will then move with the rotation-velocity rv in the direction qs. In order to determine the induction at the point q, we now give to the magnet and to the point q the velocity pv, but in a direction opposite to that previously impressed on the magnet. This comes to rest, and the point q receives a velocity equal in quantity and direction to the resultant of the two velocities pv and rv, represented in the figure by the lines qt and qs. The angle qsw being equal to the angle α, the value of that resultant will be

$$v\sqrt{r^2+p^2-2rp\cos\alpha}.$$

Now $\sqrt{r^2+p^2-2rp\cos\alpha}$ denotes the distance at which q lies from m, the point at which the magnet is fixed in the disk. Consequently the point q receives the same velocity as if it moved round the fixed magnet. As this applies to any point whatever, it follows that the induction produced in the disk during its motion round the axis will be the same as if the disk moved round the magnet with the same velocity of rotation.

The following case of induction permits the intensity of the induced current to be compared with the result obtained by calculation:—In fig. 5, sm represents a magnet having its south pole in s and its north pole in m. At the centre of the magnet and near the south pole are two brass bars of equal length, ab and cd, fixed perpendicular to the axis of the magnet. The bar ab is insulated from the magnet, and furnished at its lower side with a cylinder which is soldered to it, surrounding the magnet without being in contact with it. Upon this cylinder slides a metal spring connected to the galvanometer by the wire l. The other bar, cd, is in metallic contact with the magnet; it carries at its upper extremity a mercury-cup, into which dips the second wire, l', coming from the galvanometer.

The two brass bars are joined together by the cylinders ac and bd of the same metal. These cylinders can be placed at different distances from the magnet, either one on each side or both on the same side of it. If this apparatus be put in

rotation about the axis of the magnet so that, viewed from above, it moves in the opposite direction to the hands of a watch, an induced current is obtained passing from the mercury-cup, through the galvanometer, to the spring that slides upon the metal cylinder. We will now calculate the intensity of this induced current.

An electric element dr, situated in t, at the distance r from the magnet, has a velocity which may be designated by rv if v signifies the angular velocity. Upon this element the two poles exert equal actions, the one along the right line tu, the other along tw. Taking the component of these forces in the direction ab, we shall get ($2l$ signifying the distance between the poles, M the magnetic moment of the poles, and k a constant)

$$\frac{2k\mathrm{M}vlrdr}{(l^2+r^2)^{\frac{3}{2}}}.$$

Integrating this expression between the limits $r=0$ and $r=r_0$ (this last denoting the distance between the magnet and the cylinder ac), we get, for the total electromotive force induced in the bar ao,

$$2k\mathrm{M}v-\frac{2k\mathrm{M}vl}{(l^2+r_0^2)^{\frac{1}{2}}}. \quad . \quad . \quad . \quad . \quad . \quad (\mathrm{C})$$

The two poles tend to drive in opposite directions an electric element dz of the bar ac, situated at the distance z from the point a; but the action of the south pole is the more powerful, because that pole is nearer to dz than the other. The resultant of their action will be

$$k\mathrm{M}vr_0^2\left[\frac{1}{[(l-z)^2+r_0^2]^{\frac{3}{2}}}-\frac{1}{[(l+z)^2+r_0^2]^{\frac{3}{2}}}\right].$$

Integrating between the limits $z=0$ and $z=l$, we get, as the expression of the total of the electromotive force induced in the cylinder ac,

$$\frac{2k\mathrm{M}vl}{(l^2+r_0^2)^{\frac{1}{2}}}-\frac{2k\mathrm{M}vl}{(4l^2+r_0^2)^{\frac{1}{2}}}. \quad . \quad . \quad . \quad . \quad (\mathrm{D})$$

For the calculation of the electromotive force induced in the bar cs, the north pole will be sufficient to take into consideration, since the south pole, acting perpendicularly to the length of this bar, does not contribute to the transfer of the electric molecules. It is moreover evident that the electromotive force induced in cs acts in a direction opposite to that of the forces induced in ao and ac. The expression

of this force will be

$$+kMv-\frac{2kMvl}{(4l^2+r_0^2)^{\frac{1}{2}}}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (E)$$

Subtracting the expression (E) from the sum of (C) and (D), we get, for the total electromotive force of the induced current,

$$kMv.$$

This force, then, is independent of the distance between the bar *ac* and the magnet, just as it is of their lengths. The theoretic consideration, moreover, evidently requires that the force be independent of the section of the conductor, and that it have the same value, whether the brass cylinders *ac* and *bd* are introduced both at the same time into the apparatus, or only one at a time*. In the former case we have two electromotors; but they are placed side by side and consequently do not augment the electromotive force. The same fact presents itself here as when we connect each to each the positive poles and the negative poles of two ordinary piles: the electromotive force is not changed; but the resistance of the combined pile is lessened by one half. Now the resistance in the circuit *oacs* is extremely slight in comparison with that of the galvanometer; therefore the intensity of the current will remain the same, whether the cylinders *ac* and *bd* be both introduced into the apparatus or only one of them, and the distance at which they happen to be from the magnet will not influence the result. This is proved by the following experiments, made with an apparatus of this kind:—

	Galvanometer-deflections. scale-divisions.
A single cylinder placed at 4·5 centims. distance from the magnet	36·0 37·5
A cylinder on each side of the magnet, placed at 4·5 centims. distance . .	37·0 38·0 37·5
A single cylinder placed at 4·5 centims. from the magnet	37·3 38·0

The mean of the two first and the two last observations is 37·2; and that of the three middle observations is 37·5.

* Plücker has inferred from some experiments that the induced electromotive force of a conductor is proportional to the section of the conductor and dependent on its conductive power (Pogg. *Ann.* vol. lxxxvii. p. 368). Examination of his experiments, however, shows that they by no means authorize this conclusion.

The induced current is therefore of equal intensity with the two cylinders as with one only.

	Galvanometer-deflections. scale-divisions.	
One cylinder on each side of the magnet, at a distance of 8·5 centims. . . .	35·5 38·0 36·5	36·7
One cylinder only, at 8·5 centims. from the magnet	37·0 38·0 40·0	38·3

The mean of these six observations is 37·5, consequently nearly the same as before, though the distance from the magnet has been almost doubled.

A single cylinder at 3 centims. distance from the magnet	37·0 37·5 39·5	38·0

These observations, therefore, have verified the results of the theory.

It is unnecessary to say that the above deflections are cleared of the feeble thermoelectric currents produced by the heating of the points of contact during the rotation. It is easy to eliminate these currents, seeing that they are independent of the direction of the rotation.

After this I proceeded to the following experiment :— Two ebonite disks, *agbh* and *ekfm* (fig. 6), of equal dimensions, their peripheries encased in a band of brass, were pierced in the centre and passed over an axis *cd*, about which they could be put in rotation. *ab* and *ef* represent two brass rules fixed to the disks and communicating in *a* and *e* with the brass bands, while they are insulated from them in *b* and *f* and likewise from the axis *cd*. *sn* is a magnet passing through the two disks without any communication with the two brass rules. One of the disks is placed at the height of the south pole, and the other at the centre of the magnet. *tu* is a brass bar communicating with the rules *ab* and *ef* ; this bar can be placed at different distances on both sides of the axis. The brass springs *p* and *q* slide over the brass bands that encircle the disks, and thus communicate with the rules *ab* and *ef* and the bar *tu*. These springs are connected with the electrodes of the galvanometer. The rotation of the apparatus about *cd*, in the direction indicated by the arrow, gives rise to an electric current which passes from the spring *q*, through the galvanometer, to the other spring.

During the rotation the poles of the magnet describe circles about the rotation-axis the radius of which is equal to r_0 ;

their velocity may therefore be denoted by $r_0 v$, if v is the angular velocity. The velocity of the bar *tu* is $r_1 v$, r_1 denoting its distance from the rotation-axis. The relative velocity of the bar with respect to the magnet regarded as immovable will therefore be $(r_0 + r_1)v$, as already remarked above. If the bar passes through the points x and y on the same side of the axis of the rotation as the magnet is placed, and at the distance r_1 from that axis, the relative velocity of the bar with respect to the magnet will be $(r_0 - r_1)v$. Thus the relative velocity of the bar will in both cases be the same as if the magnet were regarded as immovable, and the disks as well as the bar as in rotation about it with the same angular velocity as that which they have in reality about the axis *cd*. It is the same with the brass rules *ab* and *ef*.

This being admitted, let us suppose we make two experiments with the same velocity of rotation, but with this difference—that in the one the bar occupies the place indicated by the figure, while in the other it is placed in *xy*. In both cases the induction will be the same as if the apparatus were in rotation about the magnet at rest. In the first case we have to consider the induction in the circuit *atue* as well as in the two rings surrounding the disks, and in the second case the induction in *axye* as well as in the same rings. Now the calculation given above has shown that the induction in the circuit *atue* is equal to that of the circuit *axye*. Therefore, if these deductions are correct, the intensity of the induced current must be the same, whatever the situation of the bar.

The experiments gave the following deflections, in divisions of the scale :—

The bar placed on the oppposite side of the axis to the magnet.		The bar on the same side of the axis as the magnet.	
$r_1 = 40$ millims.	$r_1 = 24$ millims.	$r_1 = 24$ millims.	$r_1 = 40$ millims.
17·0	17·0	15·5	16·0
16·0	16·0	17·2	17·3
16·2	15·2	17·0	16·0
Mean 16·4	16·1	16·6	16·4

As the numbers can be regarded as equal, it follows, as calculation also shows, that the intensity of the current is independent of the position of the bar.

According to the preceding statement, unipolar induction ought not to be considered a real induction, but an ordinary electrodynamic phenomenon due to the action of the magnet upon the electric currents produced by the motion of the conductor relatively to it. As for the action of the magnet, whether the currents on which it acts are produced by spe-

cial electromotive forces, or by the electric fluid being carried along in the direction of motion of the conductor, is quite immaterial. If we notwithstanding continue to regard the unipolar phenomena as belonging to those of induction, we shall be obliged to distinguish the following species of magnetic induction :—

1. Induction due to the action of the magnet upon the induced circuit augmenting or diminishing, or in general undergoing a variation. This may take place in the two following ways :

(a) The magnetic moment undergoes a change, while the induced circuit and the magnet remain at rest, and consequently do not alter their relative position ;

(b) The magnet and the induced circuit approach towards or recede from each other without the magnetic moment undergoing variation.

2. Induction due to the circumstance that the conductor moves in regard to the magnet without the distance from the poles of the latter to the different points of the conductor necessarily varying, and without augmentation or diminution of the magnetic moment—unipolar induction.

These different species of induction may of course sometimes present themselves in combination, as the following example shows. Let sn (fig. 7) be a magnet, and ab a metal tube the axis of which coincides with the prolongation of the axis of the magnet. If we increase, or if we diminish the magnetic moment, there will arise in the tube induced currents of the species 1a. If we move the brass tube towards or away from the magnet without modifying the magnetic moment, induced currents will result of the species 1b, but also at the same time those of the species 2. On removing the tube away from or approaching it to the magnet, the electric molecules present in the tube are carried in the direction of the movement, and thus form true electric currents, upon which the magnet acts according to the known law of action between a magnet and an electric current. It is easy to perceive that these two kinds of currents proceed in the same direction; but those of species 2 are so feeble in comparison with the others, that they can hardly be observed at the same time with them. In the last place, if the tube is put in rotation about its axis, induced currents exclusively of species 2 tend to be formed ; and they become appreciable if the two ends of the tube be connected with the electrodes of a galvanometer.

By making the theory which has just been expounded serve for the explanation of the known cases of unipolar induction, any one may convince himself that it gives for every case re-

sults accordant with the experiment. The principle on which it rests, that the magnet acts upon the electric currents due to the rotation of the conductor with respect to the magnet in the same way as upon ordinary currents, this principle cannot, as far as I can see, give occasion for any doubt. The only objection which it would be possible to make to it would be that, the velocity we are able to impart to the conductor being relatively slight, these currents become so feeble that the action of the magnet upon them is in reality inappreciable. But to this remark it may be replied that the velocity of the electric molecules in an ordinary galvanic current has never been measured; several physicists have assumed, upon good grounds, that this velocity is in reality not very great; it is therefore not improbable that the velocity which it is possible to impart mechanically to a conductor is comparable to that of the electric molecules in a galvanic current of average intensity. We must here carefully distinguish between the velocity with which the electric movement is propagated from place to place and that with which the molecules themselves move. These two velocities have no relation with each other; thus, as experiment shows, the former velocity may be extremely great, although the latter be insignificant*.

As the electric molecules are carried along in the direction in which the conductor is moved, it might perhaps seem, previous to mature reflection, that a cylindrical tube set in rotation about its axis must exert an electrodynamic action in the same manner as a coil through which a galvanic current passes. Such an inference, however, is any thing but justified. If we admit two electric fluids, one positive and one negative, it will be noticed that both fluids are conveyed with the same velocity and in the same direction during the movement of the conductor; therefore these curents mutually destroy their reciprocal effects. Even if it be admitted that the electric phenomena proceed from one fluid only, the above conclusion is premature. But to prove this it is necessary to revert to the principles of the theory as previously explained by me†.

The phenomena of optics have led to the assumption that the æther is attracted by ponderable matter. A material body condenses the æther between its molecules until the attraction exerted upon an æther molecule situated within the body by the molecules of the body becomes equal to the repulsion exerted by the already condensed æther upon the same æther molecule. When this point is reached the material body is no longer in a position to put in motion the external molecule of æther. Besides that quantity of æther which may be regarded

* *Théorie des Phénomènes Électriques, p.* 10. † *Ibidem.*

as connected with the body's own molecules, there exists free æther between those molecules; and the quantity of free æther contained by the entire body must be equal to the quantity of æther contained in a volume of vacuous space equal to that of the body. This is what takes place as long as the body is at rest. If, on the contrary, the body is put in motion with the æther which it contains, not only the repulsion of that æther, but also the attraction of the molecules belonging to the body upon an exterior æther molecule at rest is modified, and that in virtue of the established principle that "every thing that takes place or is effected in nature requires a certain time." But, as I have already pointed out, it does not at all follow from this principle that the modification which the attraction between the material molecules and the exterior æther molecule undergoes in consequence of the motion must be equal to the modification of the repulsion between the æther molecules contained in the body and the same exterior molecule. On the contrary, I have positively expressed the opinion * that the action between different kinds of molecules is also different in this respect. Let us now suppose that d (fig. 8) represents an element of a circuit containing the quantity m of free æther and a quantity m_0 of condensed æther, and that this element, with the æther contained in it, moves with the velocity h in the direction indicated by the arrow. We at the same time suppose that m_1 is an exterior æther molecule at rest, that the angle between the right line which joins d and m_1 and that of the direction of velocity is θ, and that the distance between the molecule m_1 and the element d is equal to r. The repulsion between m and m_1 is then, according to the memoir cited, expressed by

$$-\frac{mm_1}{r^2}(1-ah\cos\theta-\tfrac{3}{4}kh^2\cos^2\theta+\tfrac{1}{2}kh^2);$$

and the repulsion between the condensed æther m_0 and m_1 will be

$$-\frac{m_0m_1}{r^2}(1-ah\cos\theta-\tfrac{3}{4}kh^2\cos^2\theta+\tfrac{1}{2}kh^2).$$

Using a_1 and k_0 to denote new constants, the attraction exerted upon m_1 by the material molecules can be expressed by

$$+\frac{m_0m_1}{r^2}(1-a_0h\cos\theta-\tfrac{3}{4}k_0h^2\cos^2\theta+\tfrac{1}{2}k_0h^2).$$

The sum of these three expressions indicates the action of the whole of the element d upon the æther molecule m_1.

Nothing is known beforehand about the ratio existing

* *Op. cit.* p. 72.

between a and a_0, nor of that between k and k_0; they can only be determined by experiment. There is nevertheless a physical phenomenon which can make them known; and that is the development of electricity by friction. In this phenomenon the æther molecules pass from the rubber to the body rubbed, or *vice versâ*. This passage of the molecules from one side to the other can hardly have its cause except in the fact that the repulsion exerted upon them by the body in motion has been modified in some way by the motion itself. Now the action of the moving body upon an æther molecule present in the motionless rubber is given by the sum of the three expressions above cited. If now we put $\frac{a_0}{a}=\frac{k_0}{k}=\frac{m+m_0}{m_0}$, that sum will be equal to $-\frac{mm_1}{r^2}$; or, in other words, we get the same value for the repulsion as before the body was put in motion. Therefore, in order that friction may be capable of producing electricity, $\frac{a_0}{a}$ and $\frac{k_0}{k}$ cannot have precisely the same value.

What has just been said applies to the case in which the moving body and the rubber are in contact—that is to say, when r is infinitely little. If the two bodies are at a certain distance from one another, it is impossible to produce a sensible development of electricity by the movement of one of them; consequently $\frac{a_0}{a}$ and $\frac{k_0}{k}$ must be sensibly equal to $\frac{m+m_0}{m_0}$ as soon as r has a finite value. If, then, we put a body in motion together with the æther, both free and condensed, which it contains, its action upon a motionless exterior æther molecule at a given distance will be nearly as great as if the body were at rest at the same distance. It can easily be shown that the case is still the same when the exterior æther molecule is itself in motion, and consequently forms an element of a true galvanic current. A cylindrical tube put in rotation about its axis will therefore not exert any electrodynamic action; or if it does so, at least the action will be insignificant. In fact, the action of the currents due to the æther being carried in the direction of the rotation is neutralized by the opposite electrodynamic action produced by the motion of the proper molecules of the body in the same direction*.

* When deducing the formulæ expressing the reciprocal action of two galvanic current-elements while the conductors in which the electric fluid is moving are at rest, we have assumed (*op. cit.* p. 12) that $a_0=a$ and

M. Lemström, however, has succeeded in showing, in a series of very remarkable experiments, that a rotating cylinder is capable of producing a real although excessively feeble electrodynamic action. On putting a pasteboard cylinder filled with air in rapid motion round a cylinder of soft iron, he found that the magnetic condition of the latter was modified by the rotation. M. Lemström has demonstrated that the modification can hardly be attributed to any other cause than the electrodynamic action of the pasteboard cylinder. He intends to continue these researches so interesting and important, especially from the theoretic point of view.

As is sufficiently apparent from the preceding considerations, the theory above formulated for the phenomenena of unipolar induction does not rest upon vague and arbitrary assumptions respecting the properties of the magnet and the galvanic current. Quite the contrary; it is based exclusively on their known properties, discovered by means of experiments or of investigations of another kind. The theory is independent of the admission of one or two electric fluids, and gives results in complete accordance with experience and with the requirements of the mechanical theory of heat. We therefore believe we can affirm that it furnishes the only admissible and true explanation of the phenomena of unipolar induction, which, as we shall see in the following section, play a most important part in nature.

[To be continued.]

XLI. *Notices respecting New Books.*

Elements of Dynamic; an Introduction to the Study of Motion and Rest in Solid and Fluid Bodies. By W. K. Clifford, *F.R.S., late Fellow and Assistant Tutor of Trinity College, Cambridge; Professor of Applied Mathematics and Mechanics at University College, London. Part* I. *Kinematic.* London: Macmillan and Co. 1878. (Crown 8vo, pp. 221).

THIS is the first part of a work the completion of which will be looked forward to with great interest by all students of Mathematical Physics. Its scope is sufficiently described by its name: it is a treatise on Kinematics. Change of motion as it occurs in nature is due to the mutual action of bodies on each other. It is, however, possible to study and describe the motion of a system without taking into account the conditions that arise from these mutual actions. The theory of motion when thus treated is Kine-

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$k_0 = k$. In this way the same result is arrived at as by assuming that the ratio between these constants is equal to $\frac{m+m_0}{m_0}$, but its deduction becomes more simple.

matics; or, in the words of the author "the science which teaches how to describe motion accurately, and how to compound different motions together, is called *Kinematic* (*κινημα, motion*)" (p. 2). The existence of a distinction between Kinematics and Dynamics was recognized long ago, perhaps in the first instance by Ampère*; but up to the present time it has only been obtaining recognition gradually in the Mechanical Treatises most commonly in use. The Kinematical part of Applied Mechanics, it is true, received separate and systematic treatment long ago in Professor Willis's 'Elements of Mechanism;' and in Professor Rankine's 'Applied Mechanics' there is a separate Part (III.) on the "principles of Cinematics, or the comparison of Motions," in addition to a Part (IV.) on the "theory of Mechanism." But long after the publication of the former of these works the distinction was simply ignored in the text-books of Theoretical Mechanics commonly in use; or where that was hardly possible (as in treating the motion of a rigid body about a fixed point), a few kinematical propositions were introduced under some such heading as "the Geometrical Nature of a Body's Motion about a Fixed Point," or "the Geometry of the Motion of a Rigid Body," &c. In fact the distinction was not worked out completely until the appearance of the first (and hitherto the alone published) volume of Thomson and Tait's 'Natural Philosophy.'

When a subject consists of two distinct branches, that alone is a sufficient reason for their separate treatment; but if a further reason for the separation were needed, it would be found in the fact that, in consequence of the customary indirect treatment of Kinematics, parts of it seem to escape the attention of students. Thus, though in the course of their Dynamical studies most students obtain indirectly an acquaintance with the geometry of translatory and rotational motion, comparatively few (as we have reason to believe) have any acquaintance with the geometry of strains.

The work before us will doubtless confirm the existing tendency towards a separate study of the theory of pure motion: but this may be regarded as one of its least merits. The most marked peculiarity of the volume is the unusual form in which even the most elementary parts of the subject are set forth. To all appearance the author has established for himself a peculiar point of view from which to regard the whole subject of Dynamics; and consequently his exposition of the kinematical part follows lines quite wide of the beaten tracks. This does not indeed appear at first sight; for he treats it under its most obvious subdivisions of Translations, Rotations, and Strains: but when the details of any one of these subdivisions are examined the originality of the treatment becomes at once apparent. For example, the second chapter of the first book is headed "Velocities," and occupies nearly a fourth part of the volume. Although it treats in great part of matters familiar to every student of Mathematics, the form in which they appear is very unlike that in which they are commonly treated, and is apparently the author's own: *e. g.*, instead of introducing the reader

* See Whewell, 'Phil. Ind. Sci.' vol. i. p. 152.

directly to such a function as e^t, our author prefers to start with the definition "that a point is said to have logarithmic motion on a straight line when its distance from a fixed point on the line is equally multiplied in equal times" (p. 78); and then, from the discussion that follows, the function e_t, as it were, emerges. It is needless to add that it is most instructive to see how familiar matters are regarded by a mind of great acuteness. Other instances might be given, such as the articles headed "Exact Definition of Tangent," which leads up to another, entitled "Exact Definition of Velocity." However instructive many of these discussions may be, it is nevertheless somewhat difficult to account for the presence of many things in the volume. For instance, it is hard to see why a treatise on Kinematics should contain a demonstration of the fact that the flux of a function of functions is given by the formula

$$\dot{u}=\dot{x}\partial_x u+\dot{y}\partial_y u^*,$$

and the more as the demonstration is hardly perfect. What Professor Clifford says is this:—u denoting $f(x, y)$, where x and y are functions of t, we find

$$\frac{u_1-u_2}{t_1-t_2}=\frac{x_1-x_2}{t_1-t_2}.\frac{f(x_1, y_1)-f(x_2, y_1)}{x_1-x_2}+\frac{y_1-y_2}{t_1-t_2}.\frac{f(x_2, y_1)-f(x_2, y_2)}{y_1-y_2};$$

"and when we strike out common factors and omit suffixes in this last expression, it becomes $\dot{x}\partial_x f+\dot{y}\partial_y f$, where f has been shortly written instead of $f(x, y)$. Or, substituting u for f, we have" the above formula (p. 66). We will not urge that the reader might easily misunderstand the direction to strike out common factors on the right-hand side of the equation. But we would ask how, in the case of any function not purely algebraical, does the student know that there is a common factor to be struck out of the numerator and denominator of any one of the four fractions concerned? The fact is, that in all but a few cases the calculation in question can hardly be performed except by the use of limits or infinitesimals.

The point which has given rise to these remarks by no means stands alone. We find in different parts of the volume:—accounts of the elementary properties of quadric curves (pp. 27–31, 38, 39, 91) and surfaces (pp. 172–176, 177–181); the method of finding the fluxion of t^n (p. 55), of finding the area of a parabola (p. 73), of establishing the relation $\int_0^t t^k dt=t^{k+1}:k+1$ (p. 73), and of proving that "the central projection of a harmonic range is also a harmonic range" (p. 42). These and other things like them fairly suggest the question, For what class of readers is the book designed? The

* Looking at this well-known relation when thus written, we are strongly tempted to suspect that in matters of notation there is a good deal of mere fashion or arbitrary preference. Mr. Clifford's way of writing the formula combines two modes of notation, both of which, a few years ago, would have been considered to have had their day. There is, however, a reason why the fluxional notation should not be altogether lost sight of.

only answer that occurs to us is, that the writer hopes by inserting these matters to render his book accessible to readers who bring nothing with them but a knowledge of the elements of Algebra, Geometry, and Trigonometry. Our author may perhaps find one or two readers capable of making out the contents of the volume on these terms, *i. e.* capable, while studying a distinct subject, of picking up incidentally a knowledge of the Geometry of Curves and Surfaces of the second degree, the principles of the Differential and Integral Calculus, the properties of the Potential, and some thing more than the elements of Quaternions. All but a very few find it necessary to devote their undivided attention to these subjects; and even then a great amount of illustration is needed to ensure their being firmly grasped; and we may add that even the few would be the better for going through the usual discipline, though they might possibly do without it. The remarks made by a most competent judge in a somewhat similar case* would, we believe, apply to this. A retentive memory and great clearness and precision of thought may (though only in exceptional cases), supersede the necessity of a progressive training. In other cases, should the student attempt to dispense with such a training, he will probably rise from his labours without retaining a single definite conception either of the propositions or their proofs.

The Moon, her Motions, Aspect, Scenery, and Physical Condition. By Richard A. Proctor. Second Edition. London: Longmans and Co. 1878.

The appearance of a Second Edition of this work is strong evidence of an increasing interest in Selenographical inquiries. The Moon's distance, size, and motions, which Mr. Proctor treats of in his opening Chapters, have long claimed the attention of astronomers; and of all the triumphs yet achieved, the Lunar Theory stands preeminent. It is of late years that many astronomers have exhibited no little interest in examining the external characteristics of members of the Solar System, seeking from them to obtain some indications of their physical condition; and those who are interested in our Satellite are turning their attention especially to the external configuration of her globe, with a view, first, to ascertain if any changes occur among the numerous objects diversifying her surface, and, secondly, from the observations obtained to endeavour to derive some idea of the forces operating in her interior. To this part of his general subject our author has devoted two chapters—one on the study of the Moon's surface, the other on its condition. These chapters will be read with interest by every student seeking information on the rise, progress, and present state of Selenography. In many respects this edition is an improvement on the first; and we wish it all the success which the well-earned popularity of the author is likely to obtain for it.

* Peacock's Life of Young, pp. 31, 191.

XLII. *Proceedings of Learned Societies.*

GEOLOGICAL SOCIETY.

June 19, 1878.—John Evans, Esq., D.C.L., F.R.S., Vice-President, in the Chair.

[Continued from p. 235.]

THE following communications were read:—

2. "Notes on the Palæontology and some of the Physical Conditions of the Meux's-Well Deposits." By Charles Moore, Esq., F.G.S.

The author remarks that the various deep-well borings around London have abundantly proved the correctness of Mr. Godwin-Austen's inference that the Palæozoic axis of the Mendips is continued beneath the Secondary rocks of the south-eastern counties. Mr. Moore has himself shown that where these Palæozoic rocks finally disappear under the Secondary strata, there are found at the unconformable junction of the two formations a set of deposits indicating the existence of very peculiar physical conditions, and containing an admixture of fossils from very different geological horizons. Hence he was led to inquire whether any trace of similar abnormal deposits might be found in the deep-well borings of London.

With this view he set to work at washing some of the materials supplied to him from the Meux's well, and studying the minute and often microscopic organisms thus obtained.

The Chalk was not particularly examined; but from a single small sample of Upper Greensand he obtained numerous Foraminifera and Entomostraca, including one Cyprid new to science.

The Gault yielded 16 genera and over 30 species of Foraminifera, and 20 species of Entomostraca, 4 of which are new, together with many young forms of Gasteropods and Cephalopods.

But the chief interest of Mr. Moore's investigations centres in the 67 feet of strata intervening between the Gault and Devonian. In this marly and oolitic-looking deposit he found no less than 85 different kinds of organisms, exhibiting a singular admixture of marine and lacustrine forms of life. Foraminifera are rare, but Entomostraca and Polyzoa are very abundant. Some genera are found, such as *Carpenteria*, *Saccammina*, *Thecidium*, and *Zellania*, of which the range in time is greatly extended by these investigations.

The author fully confirms Mr. Etheridge's reference of the beds in question to the Neocomian period, widely as they differ in physical characters from the Lower Greensand strata of the south-east of England. From a careful study of the nature and condition of preservation of the minute organisms he concludes that the deposits which contain them were formed at first in shallow lacustrine hollows on the surface of the Devonian rocks now lying buried at a depth of 1000 feet below London, and that these lakes were invaded by the waters of the Neocomian sea, with the deposits of which their sediments were in part mingled, and under which they were finally buried.

The Chair was then taken by Prof. Prestwich, M.A., F.R.S., Vice-President.

3. "On *Pelanechinus*, a new Genus of Sea-urchin from the Coral Rag." By W. Keeping, Esq., B.A. F.G.S., Professor of Geology in the University College of Wales.

4. "Remarks on *Saurocephalus*, and on the Species which have been referred to that Genus." By E. Tulley Newton, Esq., F.G.S., of H.M. Geological Survey.

5. "A Microscopical Study of some Huronian Clay-slates." By Dr. Arthur Wichmann.

Although a considerable amount of attention has been devoted during recent years to the microscopical study of clay-slates and slate-clays, yet in none of the published researches on this subject has any account of the structure of the clay-slates of archæan age been given. The author has availed himself of the extensive series of Huronian clay-slates collected by Major T. V. Brooks in the country around Lake Superior to supply this deficiency. The succession and relation of the rocks described have been fully treated of in the work of Hermann Credner and the publications of the Geological Survey of Michigan.

The chief object of the author is to discuss the origin of the crystalline constituents in clay-slates, and at the outset he describes in detail the microscopical character of clay-slate, of novaculite or whetstone, and of carbonaceous shales and slates respectively, dwelling more especially on the crystallized minerals which can be detected in each of these rocks, and the nature of the isotropic ground-mass which sometimes surrounds them. He then points out that three theories have been advanced to account for the presence of these crystalline constituents in clay-slates. According to the first of these theories, the crystals in question are regarded as the product of chemical action in the ocean in which the original material was deposited; the second theory attributes the formation of the crystalline minerals to processes of metamorphism which have taken place subsequently to the solidification of the rocks; the third theory refers them to aggregative action going on in the still plastic clay-slate mud prior to its solidification. The first of these theories has been maintained by G. R. Credner; but against it the author adduces numerous arguments, and especially points out the difficulty of supposing an ocean capable of depositing from its waters at successive periods minerals of such different chemical composition as chlorite, actinolite, &c. In opposition to the second theory, which has received the support of Delesse, the author points out the existence in the rocks in question of broken crystals which have been recemented by the surrounding clay-slate substance. The author is thus led to incline towards the third theory, in favour of which some striking facts, drawn from the microscopical structure of the rocks, have already been adduced by Zirkel. He admits, however, that later metamorphic actions are not to be excluded in seeking to account for the origin of the crystalline constituents of

clay-slates, and points out that four distinct stages must be considered in the series of changes by which the rocks in question have acquired their present character:—1st, the deposition of the mud; 2nd, the formation of minerals during the plastic state; 3rd, the separation of materials during solidification; and 4th, the action of metamorphic processes.

6. "On a Section through Glazebrook Moss, Lancashire." By T. Mellard Reade, Esq., F.G.S.

The section described has been exposed in a cutting made by the Wigan Junction Railway. The moss rests on an almost perfectly level floor of Boulder-clay, and is at the deepest part about 18 feet thick. In the 3 or 4 feet at the base are branches &c. of trees; and the stools are found resting on and rooted in the Boulder-clay; these are of oak or birch. Prostrate trunks were found, one, an oak, being 46 feet long and 3 in diameter. The surface of the clay is about 60 feet above O.D. The author thinks the section shows that the moss originated from the decay of the forest, favoured by change of climate, and gradually extended itself from the centre outwards, trees within it at the outer part being much less discoloured than those further in. In the latter part of the paper some cuttings and borings in the clays and sands are described, and the asserted occurrence of the trunk of a tree in the Boulder-clay is noticed.

7. "On the Tertiary Deposits on the Solimões and Javary Rivers in Brazil." By C. B. Brown, Esq. With an Appendix by R. Etheridge, Esq., F.R.S., F.G.S., and communicated by him.

The author in 1874 had the opportunity of examining some beds on the Solimões, or Upper Amazon, and the Javary, one of its tributaries, containing fresh- and brackish-water shells similar to those found in Tertiary deposits at Pebas, still higher up the river. The author indicates certain errors into which he considers previous writers to have fallen, and calls attention to the great extent of these beds, now demonstrated to occupy a tract of country 300 miles in length by 50 miles in breadth, and to the enormous change in the physical features of the region which must have taken place since their deposition. When this took place the sea reached probably 1500 miles west of its present shore-line, covering the country which is now the valley of the Amazon. The absence of examples of false-bedding in the deposits leads him to the conclusion that they were formed in comparatively still water, into which flowed numerous streams bearing much vegetable matter, which has served for the formation of lignitic deposits, the whole being probably the upper beds of a series deposited under similar conditions to those of deltas in the present day. In an appendix, Mr. Etheridge notices the fossils collected by the author, which included seeds of *Chara*, and species of *Mytilus* (1 new), *Anisothyris* (4, 1 new), *Lutraria* (1), *Thracia* (1), *Anodon* (1), *Unio* (1), *Natica*? (1), *Neritina* (2 new), *Odostomia* (1), *Hydrobia* (1 new), *Isæa* (1), *Dyris* (1), *Assiminea* (1 new), *Fluella* (1), *Cerithium* (2 new), *Melania* 4(

new), and a new Gasteropod constituting a genus (*Alycæodonta*) allied to *Alycæus*. A single palatal plate of *Myliobatis* or *Zygobatis* (probably derived) was also found.

8. "On the Physical History of the English Lake-district, with Notes on the possible Subdivision of the Skiddaw Slates." By J. Clifton Ward, Esq., Assoc.R.S.M., F.G.S.

The author traces the physical history of the lake-district from the commencement of the period when the Skiddaw slate was deposited. To this succeeded the volcanic Borrowdale series, which is followed after a physical break by the Coniston Limestone. Between this and the succeeding Silurian deposits there is little, if any, break. Thus, in the Lake-district, the break between Upper and Lower Silurian is physically below the Coniston Limestone, though palæontologically it is above it.

The Old Red Sandstone period was one of denudation, which was continued into the Carboniferous period; and perhaps the whole district was actually covered by the sea during the maximum depression of the Lower Carboniferous epoch. Since then it has probably never been submerged, but exposed to continuous subaerial denudation. The physical significance of the Mell-Fell (Lower Carboniferous) conglomerates receives special attention.

The author then, from consideration of the amount of deposition and rate of denudation, attempts to estimate the period which has elapsed since the commencement of the record, and sets it down as 62,000,000 of years. The author then considers the age of the Skiddaw slates. From lithological resemblances he is led to correlate the Skiddaw grit with the basement grit in the Welsh Arenig series, and thus to regard the beds below the grit as the equivalent of the Tremadoc, and perhaps of part of the Lingula Flags.

The palæontological evidence for the correspondence of the Arenig series with the whole of the Skiddaw slates rests chiefly on Graptolites and Trilobites. The author holds that the evidence from the former is inconclusive, and that from the latter to some extent contradictory, so that the physical evidence can in no way be overridden by it.

9. "On some well-defined Life-zones in the Lower Part of the Silurian (Sedgw.) of the Lake-district." By J. E. Marr, Esq.

10. "On the Upper Part of the Bala Beds and Base of Silurian in North Wales." By F. Ruddy, Esq. Communicated by Prof. T. M'K. Hughes, M.A., F.G.S.

The author describes a series of sections in the upper part of the Bala and the succeeding beds, and gives lists of fossils. Details of the various beds between the Bala and Hirnant Limestone are given, above which come soft blue shales underlying Tarannon shales, when fossils cease until the base of the Wenlock is reached. The author has been able to trace the Hirnant Limestone and grit considerably beyond the limits of the Hirnant valley. The sections at Cynwyd (to the west of Corwen) are described. Here occur the equivalents of the Bala Limestone and beds above this up to the level (probably) of the Hirnant Grit.

XLIII. *Intelligence and Miscellaneous Articles.*

A SPECTROMETRIC STUDY OF SOME SOURCES OF LIGHT.

BY A. CROVA.

THE general law of emission of the radiations emitted by a body raised to an elevated temperature is not completely known. Dulong and Petit * have given the empirical law of the obscure radiations emanating from a body heated to temperatures below 240°; and Edm. Becquerel† has demonstrated that the intensity of the red, green, and blue radiations varies with the temperature of the body which emits them, according to an exponential law analogous to that of Dulong and Petit.

The exponentials which represent the law of emission of radiations of different refrangibilities are represented by curves of which the origin corresponds to the temperature at which the radiation considered commences to be produced, and rises the more rapidly as the wave-lengths of the radiations become less. According to M. Edm. Becquerel, the logarithms of the bases of these exponentials vary in the inverse ratio of the wave-lengths of the radiations.

These considerations may serve as a starting-point to a method of determination, *in a spectrometric way*, of the temperature of incandescent solids or liquids. In fact it follows from the investigations of Mr. Draper‡ and M. Edm. Becquerel that, when the temperature of an incandescent solid increases in a continuous manner, the spectrum of the radiations emitted by it lengthens towards the violet end, and that each of the radiations of this spectrum is at the same time increased in intensity according to an exponential formula. The temperature of the luminous source can therefore be measured:—(1) by means of the wave-length of the radiation which limits the spectrum towards the violet; (2) by the position of the thermal maximum of the spectrum, which approaches nearer to the violet in proportion as the emission-temperature becomes higher; (3) by means of the ratio of the luminous intensity of a determinate radiation λ, taken in the spectrum of the source, to the intensity of the same radiation in the spectrum of a source of known temperature, compared with the ratio of the luminous intensities of another radiation λ' in the same two spectra.

These last determinations can be easily effected by means of a spectrophotometer. Several observers have made use of instruments of this kind §. I used that of M. Glahn, which permits measurements to be made upon homogeneous radiations.

On the other hand, I have measured the thermal intensity of the simple radiations of the solar spectrum by means of a linear thermoelectric pile and a very sensitive galvanometer, using for the

* *Ann. de Chimie et de Physique*, 2e série, t. vii.

† Edm. Becquerel, *La Lumière*, t. i. pp. 61–67.

‡ Phil. Mag. 1847, vol. xxx. p. 345.

§ Govi, *Comptes Rendus*, t. l. p. 156 (1860). Trannin, *Journal de Physique*, t. v. p. 297. Vierordt, Pogg. *Ann.* 5th series, vol. xx. Glahn, Wiedemann's *Annalen*, vol. i. (1877).

first trials a flint-glass prism and a glass concave mirror silvered at its surface instead of an achromatic lens. The employment of a network of lines engraved on the metal instead of the prism would permit the influence of any elective absorption to be eliminated.

I have made numerous determinations of thermal curves of the solar spectrum on exceptionally fine days, at different periods in the years 1877 and 1878. These curves differ in the ratio of their respective ordinates, but especially in the position of the thermal maximum, as has been shown by Melloni. These curves were rendered comparable with one another by bringing them to the scale of the wave-lengths, and reducing, by means of the dispersion-curve of the prism, the intensities to those which would correspond to the theoretic case of the normal spectrum—that is, of constant dispersion.

The following are, for the luminous part of these spectra, the means of a number of concordant observations made under excellent atmospheric conditions:—

	millim.	millim.	millim.	millim.	millim.	millim.
Wave-lengths	0·000676	0·000605	0·000560	0·000523	0·000486	0·000459
Intensities	1000	820	760	670	540	460

I have represented by 1000 the thermal intensity which corresponds to a red radiation of wave-length 0·000676 millim.; the intensities measured in the ultra-red cannot find a place in this Table, the corresponding wave-lengths not being accurately known.

Now here are the ratios of the luminous intensities of the same radiations of the spectra of the following sources, compared with the light of the sun:—

	millim.	millim.	millim.	millim.	millim.	millim.
Wave-lengths	0·000676	0·000605	0·000560	0·000523	0·000486	0·000459
Electric light	1000	707	597	506	307	228
Drummond light...	1000	573	490	299	168	73
Moderator lamp...	1000	442	296	166	80	27

The electric light was from 60 large Bunsen elements, Foucault regulator, with M. Carré's carbons in the focus of a metallic concave mirror; the Drummond light, oxygen and illuminating-gas thrown upon lime; the moderator lamp, fed with colza-oil. I measured the ratio of the intensity of each of the radiations of these spectra, corresponding to the wave-lengths of the preceding Table, to the intensity of the same radiation in the solar spectrum, representing these latter by the value of their thermal intensities, and always representing by 1000 the intensity corresponding to the wave-length 676.

For luminous radiations which have undergone no weakening by previous transmission, there would be proportionality between the thermal and luminous intensities of one and the same radiation, whatever its origin, as MM. Jamin and Masson have demonstrated; but the experiments of M. Desains* have shown that, in the con-

* *Comptes Rendus*, t. lxvii. p. 297.

trary case, rays of the same wave-length, taken from different spectra, may have notably different properties.

We can, however, already state that, the intensity being the same in the red for the four spectra, the weakening towards the violet varies with each source, according to a certain function of the temperature; and without being able yet to attempt a measurement of this, we can already arrange them in the order of increasing temperatures:—moderator lamp; stearine candle; illuminating-gas, of which I have not given the less-accordant Tables; Drummond light; electric light, and, lastly, the solar light, which corresponds to an emission-temperature much higher than that of the electric light, in spite of the uncertainty caused by the absorptions it has undergone from its transmission through the gaseous envelopes of the sun and our atmosphere.

It will be possible to make rigorously exact measurement of the temperatures in the spectrometric way as soon as we know the precise law of emission for all the radiations and the numerical constants for each wave-length. The results contained in this Note may be regarded as a first essay towards the solution of this important question.—*Comptes Rendus de l'Académie des Sciences*, August 19, 1878, tome lxxxvii. pp. 322–325.

ON THE EXCITATION OF ELECTRICITY BY PRESSURE AND FRICTION. BY H. FRITSCH, OF KÖNIGSBERG, PRUSSIA.

1. It is well-known that many crystalline bodies can be powerfully electrified by pressure. This, however, takes place in each case only under a perfectly definite condition. Calc-spar becomes electric only when pressed against another substance, never when pressed against another piece of calc-spar. Three pieces of calc-spar were laid one upon another; a pressure was then exerted upon the uppermost, which would have made each of the pieces singly distinctly electric: the central piece proved to be quite devoid of excitation; only the two outer ones possessed the usual quantity of electricity. If two calc-spars were pressed against one another, the surfaces which were in contact with the foreign bodies by which the pressure was exerted exhibited electricity distinctly; the two inner surfaces, where calc-spar had been in contact with calc-spar, were without excitation. I have not yet succeeded in carrying out the same experiment with other bodies.

2. According to previous observations, a definite substance, on undergoing friction against another, acquires always a certain invariable electrical excitation independent of the collateral circumstances, and accordingly the nature of the electricity excited is constantly the same. To test the correctness of this position the following experiments were instituted, in which the collateral circumstances were varied as much as possible.

a. With a violin-bow I stroked plates of zinc, copper, brass, and

four different glasses so that they vibrated transversally; they became only *negatively* electrified. If the same bow was drawn backwards and forwards lengthwise along the same part of the plate without producing a tone, it became only *positively* electric.

b. Copper plates of 4 and 7 centims. diameter were whipped with white silk in various ways. If the stroke was delivered nearly perpendicular to the plate, the latter became strongly *positive*; if it was more nearly grazing, the plate became just as strongly *negative*. Whether the plate was fresh cleaned with hydrochloric acid, or by longer exposure to the air had become tarnished, made no difference, nor yet the size of the plate. Further, by *lightly* rubbing its entire rim (best with a silk or woollen cloth in the form of cone-cover), the plate is always rendered *negative*; by *hard* rubbing with the same silk on the same place, always *positive*. Coarse woollen cloth appeared to excite less powerfully. Zinc gave the same result.

c. With brass the collateral circumstances appeared to have an influence. A square plate intended for the production of Chladni's sound-figures behaved exactly like the copper plate. The shallow brass scale of a table-balance, however, gave both electricities only when struck with silk, and when its surface was well cleaned. Lastly, an old pound-weight could not be excited in different ways with silk; this could only be accomplished with a violin-bow, according as the bow was applied to its thick main part, or to its thin neck.

d. A small scale-pan of fine silver gave both electricities only with silk, with wool it only became negatively electric.

e. A hard-gum plate always became *negative* when *slowly* stroked with a tightly folded linen cloth—when stroked *quickly*, in otherwise like circumstances, *positive*. The surface of the hand produced the same effect as linen; only for the positive excitation the stroking had to be very rapid.

f. White silk always makes the principal-cleavage-surface of gypsum *positive*, but the second, which exhibits a vitreous lustre, *negative*; while it makes no difference whether the surfaces of this second cleavage are already present on the piece of gypsum, or are artificially produced by roughly scraping a surface of the principal one.

Many other substances give opposite electric excitation on friction, according to the circumstances—for instance, mica struck with silk, hardgum rubbed with copper, hardgum whipped with silk, glass struck with silk; I have not, however, succeeded in discovering positive rules for this. The few experiments cited above (from *a* to *f*) even show the impossibility of constructing a series of intensities for frictional electricity. If two bodies are rubbed against one another, the electricity excited in each of them may change into the opposite as the pressure, the velocity, or the direction of the rubbing motion, &c. varies.—Wiedemann's *Annalen*, 1878, No. 9.

THE SOLAR ECLIPSE OF JULY 29TH, 1878.

BY PROFESSOR HENRY DRAPER, M.D.

As I have recently been giving attention to the subject of solar spectroscopy in consequence of my discovery of oxygen in the sun, it seemed to be desirable to take advantage of the total eclipse of July 29th, to gain as precise an idea as possible of the nature of the corona, because the study of that envelope has been regarded as impossible at other times. The main point to ascertain was whether the corona was an incandescent gas shining by its own light, or whether it shone by reflected sunlight.

For this purpose I organized an expedition, and was fortunate enough to secure the cooperation of my friends Professors Barker and Morton and Mr. Edison. The scheme of operations was as follows:—(1) the photographic and photo-spectroscopic work as well as the eye slitless spectroscope were to be in charge of my wife and myself; (2) the analyzing slit spectroscope was in charge of Prof. Barker, with the especial object of ascertaining the presence of bright lines or else of dark Fraunhofer lines in the corona; (3) the polariscopic examinations were confided to Professor Morton, who was also to spend a few moments in looking for bright or dark lines with a hand spectroscope; (4) Mr. Edison carried with him one of his newly invented tasimeters with the batteries, resistance coils, Thomson's galvanometer, etc., required to determine whether the heat of the corona could be measured.

This entire programme was successfully carried out; and good fortune attended us in every particular. The results obtained were:—1st, the spectrum of the corona was photographed, and shown to be of the same character as that of the sun, and not due to a special incandescent gas; 2nd, a fine photograph of the corona was obtained, extending, in some parts, to a height of more than twenty minutes of arc—that is, more than 500,000 miles; 3rd, the Fraunhofer dark lines were observed by both Professors Barker and Morton in the corona; 4th, the polarization was shown by Professor Morton to be such as would answer to reflected solar light; 5th, Mr. Edison found that the heat of the corona was sufficient to send the index beam of light entirely off the scale of the galvanometer. Some negative results were also reached, the principal one being that the 1474K, or so-called corona line, was either very faint or else not present at all in the upper part of the corona, because it could not be observed with a slitless spectroscope, and the slit spectroscope only showed it close to the sun.

The general conclusion that follows from these results is, that on this occasion we have ascertained the true nature of the corona, viz.:—it shines by light reflected from the sun by a cloud of meteors surrounding that luminary, and that on former occasions it has been infiltrated with materials thrown up from the chromosphere, notably with the 1474 matter and hydrogen. As the chromosphere is now quiescent, this infiltration has taken place to a scarcely perceptible degree recently. This explanation of the

nature of the corona reconciles itself so well with many facts that have been difficult to explain, such as the low pressure at the surface of the sun, that it gains thereby additional strength.

The station occupied by my temporary observatory was Rawlins (latitude 41° 48′ 50″, longitude 2^h 0^m 44^s W. of Washington, height 6732 feet above the sea), on the line of the Union Pacific railroad, because, while it was near the central line of totality, it had also the advantages of being supplied with water from the granite of the Cherokee Mountain and of having a repair-shop where mechanical work could be done. I knew by former experience that the air there was dry and apt to be cloudless: in this particular our anticipations were more than fulfilled by the event; for the day of totality was almost without a cloud, and the dew-point was more than 34° F. below the temperature.

The instruments we took with us were as follows, and weighed altogether almost a ton:—(1) An equatorial mounting with spring governor driving-clock, loaned by Professor Pickering, Director of Harvard Observatory. (2) A telescope of five and a quarter inches aperture and seventy-eight inches focal length, furnished with a lens specially corrected for photography, by Alvan Clark & Sons. (3) A quadruple achromatic objective of six inches aperture and twenty-one inches focal length, loaned by Messrs. E. and H. T. Anthony, of New York; to this lens was attached a Rutherfurd diffraction grating nearly two inches square, ruled on speculum-metal. The arrangement, with its plate-holders, etc., will be designated as a phototelespectroscope. (4) A four-inch achromatic telescope with Merz direct-vision spectroscope, brought by Professor Barker from the collection of the University of Pennsylvania. (5) A four-inch achromatic telescope, also brought by Professor Barker; to it was attached Edison's tasimeter. Besides these there were polariscopes, a grating spectroscope, an eye slitless spectroscope with two-inch telescope, and, finally, a full set of chemicals for Anthony's lightning collodion process, which in my experience is fully three times quicker than any other process.

The arrangement of the phototelespectroscope requires further description; for success in the work it was intended to do, viz. photographing the diffraction spectrum of the corona, was difficult, and in the opinion of many of my friends impossible. In order to have every chance of success, it is necessary to procure a lens of large aperture and the shortest attainable focal length, and to have a grating of the largest size adjusted in such a way as to utilize the beam of light to the best advantage. Moreover the apparatus must be mounted equatorially and driven by clockwork, so that the exposure may last the whole time of totality; and the photographic work must be done by the most sensitive wet process. After some experiments during the summer of 1877 and the spring of 1878, the following form was adopted.

The lens being of six inches aperture and twenty-one inches focal length, gave an image of the sun less than one quarter of an inch in diameter and of extreme brilliancy. Before the beam of light

from the lens reached a focus it was intercepted by the Rutherfurd grating set at an angle of sixty degrees. This threw the beam on one side and produced there three images—a central one of the sun and on either side of it a spectrum; these were received on three separate sensitive plates. One of these spectra was dispersed twice as much as the other—that is, gave a photograph twice as long. This last photograph was actually about two inches long in the actinic region. If, now, the light of the corona was from incandescent gas giving bright lines which lay in the actinic region of the spectrum, I should have procured ring-shaped images, one ring for each bright line. On the other hand, if the light of the corona arose from incandescent solid or liquid bodies, or was reflected light from the sun, I was certain to obtain a long band in my photograph answering to the actinic region of the spectrum. If the light was partly from gas and partly from reflected sunlight, a result partly of rings and partly a band would have appeared.

Immediately after the totality was over and on developing the photographs, I found that the spectrum-photographs were continuous bands without the least trace of a ring. I was not surprised at this result, because during the totality I had the opportunity of studying the corona through a telescope arranged in substantially the same way as the phototelespectroscope, and saw no sign of a ring.

The plain photograph of the corona taken with my large equatorial on this occasion shows that the corona is not arranged centrally with regard to the sun. The great mass of the matter lies in the plane of the ecliptic, but not equally distributed. To the eye it extended about a degree and a half from the sun towards the west, while it was scarcely a degree in length towards the east. The mass of meteors, if such be the construction of the corona, is therefore probably arranged in an elliptical form round the sun.—Silliman's *American Journal*, September 1878.

WATSON'S INTRA-MERCURIAL PLANET.

To the Editors of the Philosophical Magazine and Journal.

Haverford College, Pennsylvania,
September 9, 1878.

GENTLEMEN,

Gaillot's estimated orbit for Watson's inner planet (C. R. 5 Août 1878) accords closely with my predictions in 1873 (Proc. Soc. Phil. Amer. xiii. pp. 238, 472). It appears to be the third of the harmonically indicated intra-Mercurial planets.

	Mean distance.	Time.
Gaillot (computed)	·164	24·25 days.
Chase (predicted)	·165	24·50 „

Yours truly,

PLINY EARLE CHASE.

THE

LONDON, EDINBURGH, AND DUBLIN

PHILOSOPHICAL MAGAZINE

AND

JOURNAL OF SCIENCE.

[FIFTH SERIES.]

NOVEMBER 1878.

XLIV. *On the Experimental Determination of Magnetic Moments in Absolute Measure. By* THOMAS GRAY*, *B.Sc., Thomson Experimental Scholar in the University of Glasgow* †.

[Plate V.]

SOME experiments on the intensity of the earth's magnetism were made by Gauss, and published, under the title "Intensitas Vis Magneticæ Terrestris ad Mensuram Absolutam Revocata," in the *Commentationes Societatis Gottingensis*, 1832, and in a paper on the General Theory of Terrestrial Magnetism, an English version of which is given in the second volume of 'Taylor's Scientific Memoirs.'

The results are given (according to the system the importtance of which was first seen, and which was first introduced, by Gauss himself) in absolute measure. The units of length, mass, and time employed by him are the millimetre, milligramme, and second. His results, when reduced to C.G.S. units, show that a steel magnet with which he experimented had a magnetic moment of 22·2 per gramme mass.

Gauss also calculated (Taylor's Scientific Memoirs, vol. ii. p. 225) the mass of steel which would have to be placed in each cubic metre of non-magnetic matter in order to make up

* Now Demonstrator in Physics and Instructor in Telegraphy in the Imperial College of Engineering, Tokei, Japan.

† Being the Essay to which the Cleland Gold Medal was awarded by the University of Glasgow in the Session 1877–78. Communicated by Sir W. Thomson to the Philosophical Magazine by permission of the Senate.

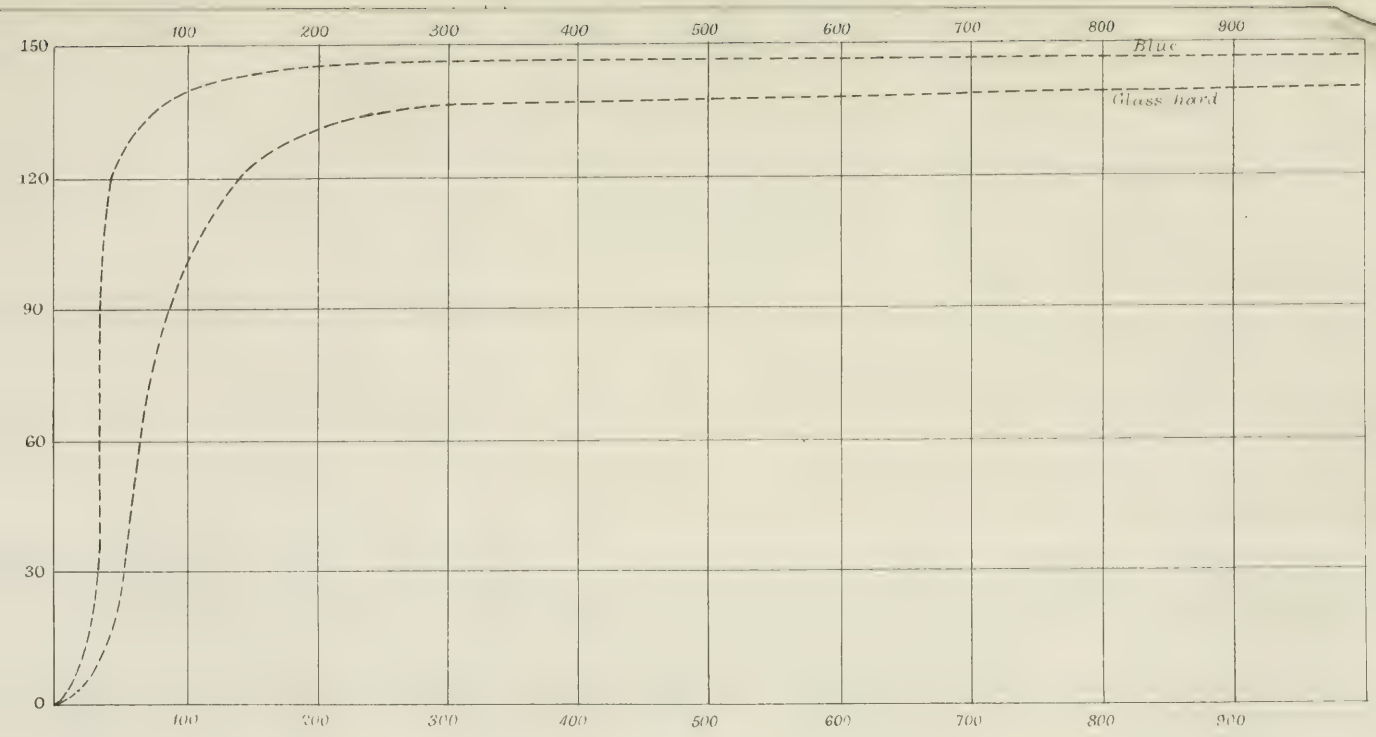
a globe of the same magnetic moment as the earth. He found that this bar must contain 3·55 kilogrammes of the steel of which his magnets were made.

No experiments, however, have been made and published hitherto (so far as I know) having for their object the determination of the magnetic moments of steel magnets of different tempers and tempered by different methods, or which give information as to the permanence or non-permanence of the magnetism of such bars when left undisturbed for any considerable time. The experiments described below were undertaken with the view of supplying some approximately accurate information on these points, and also as to whether a hard or soft quality of steel gave the stronger magnets. They were performed in the Physical Laboratory of the University of Glasgow. The experiments on the effect of temper were all made on small bars cut from a wire of soft carbon steel.

The apparatus used is shown in the accompanying diagram (Pl. V.). M represents the magnetometer, which is a reflecting instrument consisting of a small mirror about one centimetre in diameter, carrying, cemented to its back, four small needles about 0·8 centimetre long, and suspended by a single silk fibre ten centimetres long, which passes down a narrow slit cut in the front of a wooden upright fixed to the base. This slit terminates in a small cell, in which the mirror hangs. The slit and cell being closed in front by a glass plate, a dead-beat arrangement is obtained similar to that of Thomson's reflecting galvanometer. B B is a bar of wood capable of turning round the vertical axis R, which, by means of a brass spring S is made to bear against two brass V's, one of which is fixed to the upper and the other to the under side of B B. A A are two arms of wood (shown in plan at the foot of the diagram), each of them fixed to B B by means of two thin wooden strips W. As will be seen from the plan of the arms, these strips were, in every position in which they were placed, in a vertical plane passing through the axis R.

The upper side of the bars A A was on a level with the centre of the mirror; and along the centre of them a small V-groove was cut, the line of which was arranged to pass through the centre of the mirror. The axis of a magnet placed in this groove could evidently, by turning the arms A A, be caused to make any desired angle with the magnetic meridian; and hence the instrument could be used either as a sine or tangent instrument. L is an ordinary galvanometer-lamp, and M a scale of half-millimetres placed at a distance of one metre from the plane of the mirror.

The image of a fine wire, fixed vertically at F, was brought



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to a focus on the scale by sliding L to the proper distance from the mirror, and served as an index by means of which the deflection was read. The distance of any point on either of the arms A A from the mirror was found by referring it to a fine line marked on the upper surface of each of the arms by a sharp point attached to a fixed support above in such a position as just to bear on the upper surface of the arms when they were turned under it. The distance of this line from the axis was evidently the same for each of the arms; and half the total distance between the two lines thus drawn gave the distance of either.

The plane of the magnetometer-needles was made to pass through the axis by first placing the magnetometer in such a position on the stand that this condition was approximately fulfilled, and then adjusting it by means of the levelling-screws at the base of the instrument until the deflections given on the scale by a magnet placed in the V-groove on one of the arms, when the arm was turned so that the magnet was alternately due east and due west of the centre of the mirror, were equal. In order that the magnetometer might be removed from the stand when desired, and replaced in exactly the same position, Sir William Thomson's geometrical arrangement was employed. One of the three rounded feet of the instrument was made to rest in a conical hollow cut in the upper surface of the stand, another in a V-groove cut with its axis in line with the centre of the conical hollow, and the remaining foot on the plane upper surface of the stand.

The mode of experimenting was as follows:—A large number of cylindrical steel bars were cut from the same bar, the diameter of which was ·097 centimetre, its weight per metre 5·77 grammes, and its density 7·83.

Before being tempered the bars were carefully filed to a uniform length of five centimetres. Their lengths were compared with a scale of half-millimetres by means of a lens. About sixty of these bars, in order that they might be heated as nearly as possible to the same temperature, were spread on the bottom of a small thin iron tray, and the whole raised to a bright red heat in the heart of a glowing fire. To temper the bars glass-hard, the tray with its contents was quickly removed from the fire and plunged into water at about 15° Centigrade. The bars were then made up into parcels of five each, and placed in a vessel containing oil. The whole was then heated by means of a Bunsen lamp, and parcels of the bars removed at each of the following temperatures—100°, 150°, 200°, 240°, 250°, 260°, 270°, 280°, 300°, 310°. While this process was going on, the heated oil was taken advantage of to temper a number

of separate parcels by plunging them, after having been heated to a bright red heat, into oil of various temperatures. These bars were not again heated. All the bars were then magnetized by the current from ten of Thomson's Tray Daniells, flowing through a magnetizing coil, of the same length as the bars, made of insulated copper wire. This coil consisted of four layers of forty turns each, and had a resistance of ·065 ohm.

Thus, taking the electromotive force of a Tray Daniell as 10^8 C.G.S. units, and its resistance as ·1 ohm or 10^8 C.G.S. units, the current was, in absolute measure, approximately, $\frac{1}{1\cdot065}$. This was distributed over 160 turns of a solenoid five centimetres long; and therefore the magnetizing force was $\frac{1}{1\cdot065}\times\frac{160}{5}\times 4\pi = 377$ nearly.

Before the magnetic moments could be calculated, it was of course necessary to determine the horizontal component of the earth's magnetic force at the place where the magnetometer was to stand during the experiment. This was done by observing the period of oscillation, under the horizontal component of the earth's force, of five separate magnets, each suspended by a silk fibre about thirty centimetres long, and enclosed in a glass case placed on the magnetometer-stand. The magnetometer was then placed on its stand, and the deflection of its needle by each of these magnets, placed with its centre at a distance of twenty centimetres from the needle, observed. A reading was taken with the magnet resting in the groove on the arm A, which was placed in an east-and-west direction. The arm was then turned through 180°, and a reading taken with the magnet in its new position.

The same operations were repeated with the ends of the magnet reversed, and the arithmetic mean of these four readings taken as the deflection on the scale.

The formulas for deducing the horizontal component and the magnetic moment are easily obtained, as follows :—

Let H = horizontal component of the earth's magnetic force.
T = period of vibration of magnet under H.
μ = moment of inertia of magnet round an axis through its centre at right angles to its length.
r = distance of centre of magnet from centre of needle.
a = virtual half-length of the magnet (that is, half the distance beteen its poles).
θ = deflection of needle in degrees.
M = moment of magnet.

Then for equilibrium we have

$$\frac{M}{2a}\left\{\frac{1}{(r-a)^2}-\frac{1}{(r+a)^2}\right\}=H\tan\theta;$$

therefore

$$M=\frac{(r-a)^2\,(r+a)^2}{2r}\,H\tan\theta.$$

Hence

$$\frac{H}{M}=\frac{2r}{(r-a)^2\,(r+a)^2\tan\theta}.$$

Again,

$$T^2=\frac{4\pi^2\mu}{M\,H},$$

or

$$H=\frac{4\pi^2\mu}{T^2\,M}.$$

Substituting the value found above for M and squaring, we get

$$H^2=\frac{8\pi^2\mu\,r}{T^2\,(r-a)^2\,(r+a)^2\tan\theta}.$$

These formulas were used in preference to the approximate formulas which they become when a is struck out, and which are generally employed.

The distance between the poles of a magnet, or its virtual length $2a$ in the above formulas, may be determined by observing the deflections θ and θ' of the magnetometer-needle produced by the magnet when placed at distances r and r' from the centre of the needle.

For we have the equations

$$\frac{2\,M}{H}=\frac{(r^2-a^2)^2\tan\theta}{r},$$

$$=\frac{(r'^2-a^2)^2\tan\theta'}{r'},$$

from which we obtain by reduction

$$a^2=\frac{r'^2\sqrt{r\tan\theta'}-r^2\sqrt{r'\tan\theta}}{\sqrt{r\tan\theta'}-\sqrt{r'\tan\theta}}.$$

The average of a number of determinations of a made by this method agreed almost exactly with the actual half-length of the magnet; and as the effect of a slight error in the value of a does not sensibly affect the value of M, the actual half-length was used in all the calculations.

The values found from each of the five magnets are given in the following Table. These results, as well as all those which follow, are given in C.G.S. units.

No. of Magnet.	Value of H.
1.	·15379
2.	·15412
3.	·15422
4.	·15405
5.	·15375

Mean value ·15399.

In the determination of the magnetic moments no correction of the value of H was made for induction.

The magnets were then magnetized a second time by means of the current from twenty double tray-cells flowing through a magnetizing coil of the same length as the magnets, consisting of ten layers of wire of seventy turns each, and having a resistance of 2·15 ohms. By the same method as before, the magnetizing force is found to be 1100 nearly. For ease of comparison, the moments of the magnets after the first and second magnetization are placed side by side in the Tables of results. They show that the magnetizing force first employed nearly saturated the bars. A few of these magnets were placed between the poles of a powerful Ruhmkorff magnet, when the increase of magnetism was found to be very small. It will be observed from the Tables of results that one or two of the magnets showed that their magnetic moment had been diminished by the second magnetization. This may have been due to some accident; but the results have been entered in the Tables as they were obtained.

To show the relative effects of different magnetizing forces on bars tempered glass-hard and "blue," two bars were brought one to each of these tempers, and were then magnetized, first with a very small magnetizing force, which was increased by small measured amounts, and observations of the magnetic moments of the bars made at each increase. The results are shown in the annexed curves (Plate V.), of which the upper corresponds to the blue-tempered, and the lower to the glass-hard magnet. The abscissæ are proportional to magnetizing forces, and the ordinates to magnetic moments. It will be observed from the curve that for every magnetizing force the magnetic moment of the blue-tempered magnet is greater than that of the glass-hard magnet, and that the difference between them diminishes as the magnetizing force is increased.

As will seen from the appended Tables, the results show that magnets made of steel which had been previously heated to a bright red and cooled suddenly in cold water, were scarcely so strong, after the first magnetization, as those (made of the same steel) which, after having been so treated,

had been again heated in oil to any temperature up to 310° Cent., and afterwards allowed to cool slowly in air. By the second magnetization, however, the difference between the magnetic moments per gramme of the glass-hard and softer magnets, though still in favour of the latter, was greatly diminished, which seemed to indicate that the greater strength of the softer magnets after the first magnetization was rather due to their being more easily magnetized to saturation.

Magnets made of steel which had been heated to redness and then cooled in oil had a comparatively small magnetic moment when they had been cooled in cold oil; but the magnetic moments gradually increased as the temperature of the oil was raised till it reached about 150° Cent., after which the magnetic moments were smaller the higher the temperature of the oil. This result was not altered by the second magnetization. The magnetic moments of these bars varied from about 60 to 80 per gramme.

Some magnets supplied to Mr. James White, Glasgow, to be used as adjusting magnets for Sir William Thomson's compass, were found to have, when magnetized by a powerful Ruhmkorff coil, an average magnetic moment of about 50 per gramme. Each bar weighed about 170 grammes, and was 30 centimetres long.

These magnets, when supplied by the maker, were comparatively soft; and their magnetic moments were slightly diminished by tempering them glass-hard and magnetizing them.

A series of experiments was made with magnets of the homogeneous iron or steel supplied by Webster and Horsfall for the sheathing of the 1865 cable. Each of these magnets was five centimetres long, and weighed 2·27 grammes. Their magnetic moments per gramme for the different tempers were as follows:—

Glass-hard	20·22
Yellow	17·18
Blue	11·29
As supplied.....	12·09

This shows a marked difference as to magnetic moment between magnets made of this steel and the magnets used in the former experiments.

Thus the glass-hard magnets of the soft steel had a magnetic moment of 74·3 per gramme, while the magnetic moments at the other tempers were a little greater. On the other hand, as the Table above shows, the magnetic moment of the glass-hard magnets made of Webster and Horsfall's steel

was only 20·22, and that of the blue-tempered magnets little more than half of this amount.

With regard to the permanence of the magnets employed in the first series of experiments, their magnetic moments were found to be very little, if at all, changed by lying nine months undisturbed in the laboratory. When newly magnetized, the glass-hard magnets lost about 2 per cent. of their magnetic moment when allowed to fall three times with true north pole down from a height of 70 centimetres to a hard paving-stone. Blue-tempered magnets by the same treatment lost 10 per cent. After lying nine months, the glass-hard magnets when allowed to fall three times as just described, lost 0·5 per cent. of their magnetic moment, blue 2·8 per cent.

The same magnets were then remagnetized and again allowed to fall as before. The magnetic moment, which had been but little increased, was found to be diminished in the case of those tempered glass-hard by 1·7 per cent., and in the case of those tempered blue by 4·4 per cent.

Description of temper.	No. of magnet.	First magnetization.		Second magnetization.	
		Total magnetic moment.	Magnetic moment per gramme mass.	Total magnetic moment.	Magnetic moment per gramme mass.
Heated to bright redness, and plunged in water at about 15° C. (Glass-hard.)	1.	20·496	72·296	21·129	74·529
	2.	20·551	71·857	21·204	74·140
	3.	20·551	72·235	21·129	74·296
	4.	20·291	71·322	21·185	74·464
	5.	20·328	71·415	21·073	74·201
Mean	.....	20·44	71·82	21·14	74·33
Glass-hard, reheated to 100° C., and cooled in air.	6.	21·054	76·566	21·304	77·477
	7.	20·568	75·897	21·054	77·660
	8.	20·869	75·887	21·258	77·275
	9.	20·869	75·203	21·285	76·670
	10.	20·308	74·651	20·626	75·800
Mean	.....	20·73	75·64	21·10	76·98
Glass-hard, reheated to 150° C. and cooled in air.	11.	20·496	76·107	20·663	76·675
	12.	20·681	76·032	21·017	77·265
	13.	21·092	76·696	21·296	77·440
	14.	20·253	74·323	20·701	75·962
	15.	20·421	74·834	20·504	75·175
Mean	.....	20·59	75·60	20·73	76·50

Table (*continued*).

Description of temper.	No. of magnet.	First magnetization.		Second magnetization.	
		Total magnetic moment.	Magnetic moment per gramme-mass.	Total magnetic moment.	Magnetic moment per gramme mass.
Glass-hard, reheated to 200° C. and cooled in air.	16.	20·103	73·637	20·328	74·461
	17.	20·794	76·029	20·818	76·117
	18.	20·999	76·221	21·352	77·499
	19.	20·869	75·886	21·072	76·627
	20.	21·185	77·036	20·756	75·476
Mean	.....	20·79	75·76	20·87	76·04
Glass-hard, reheated to 240° C. and cooled in air. (Yellow.)	21.	20·031	74·604		
	22.	21·465	78·623		
	23.	21·611	77·947		
	24.	21·222	78·600		
	25.	21·109	77·322		
Mean	.....	21·09	77·42		
Glass-hard, reheated to 250° C. and cooled in air. (Brown-yellow.)	26.	19·810	73·506	20·143	74·742
	27.	21·465	78·197	21·465	78·197
	28.	21·204	78·243	21·314	78·649
	29.	21·222	77·452	21·244	77·533
	30.	20·762	77·037	20·818	77·251
Mean	.....	20·89	76·89	21·00	77·27
Glass-hard, reheated to 260° C. and cooled in air. (Brown.)	31.	20·031	73·915		
	32.	20·907	75·888		
	33.	20·557	74·889		
	34.	21·908	79·956		
	35.	20·496	74·939		
Mean	.....	20·78	75·92		
Glass-hard, reheated to 270° C. and cooled in air. (Purple-brown.)	36.	20·346	74·664		
	37.	21·595	78·243		
	38.	21·744	79·358		
	39.	21·279	77·182		
	40.	21·285	78·508		
Mean	.....	21·25	77·59		
Glass-hard, reheated to 280° C. and cooled in air. (Purple.)	41.	20·464	75·794		
	42.	21·379	78·744		
	43.	20·391	74·151		
	44.	20·765	76·202		
	45.	20·895	77·676		
Mean	.....	20·77	76·51		

Table (*continued*).

Description of temper.	No. of magnet.	First magnetization.		Second magnetization.	
		Total magnetic moment.	Magnetic moment per gramme mass.	Total magnetic moment.	Magnetic moment per gramme mass.
Glass-hard, re-heated to 290° C. and cooled in air. (Light blue.)	46.	20·531	75·757		
	47.	20·291	75·713		
	48.	20·254	74·736		
	49.	20·464	75·372		
	50.	20·409	79·146		
Mean	.....	20·39	—76·14		
Glass-hard, re-heated to 300° C. and cooled in air. (Full blue.)	51.	20·464	75·097	21·017	77·125
	52.	21·389	79·220	21·504	79·644
	53.	22·080	80·732	21·634	79·099
	54.	21·595	78·812	21·702	79·206
	55.	21·519	79·115	21·866	80·389
Mean	.....	21·41	78·60	21·54	79·09
Glass-hard, re-heated to 310° C. and cooled in air. (Very dark blue.)	56.	20·849	78·674		
	57.	20·979	79·316		
	58.	21·036	80·014		
	59.	21·379	81·444		
	60.	22·248	76·764		
Mean	.....	21·30	79·24		
Heated to a bright red heat and cooled in boiling water. (Glass-hard.)	61.	17·534	61·308	17·944	62·741
	62.	18·987	66·272	18·819	65·686
	63.	18·334	63·993	18·449	64·394
	64.	19·936	68·871	19·930	68·843
	65.	19·025	65·605	19·597	67·576
Mean	.....	18·76	65·21	18·95	65·85
Heated to a bright red heat and cooled in cold oil.	66.	17·888	63·433	17·986	63·780
	67.	17·551	62·127	17·615	62·354
	68.	17·011	60·216	17·615	62·354
	69.	17·546	62·110	17·059	60·385
	70.	17·589	62·151	17·096	60·410
Mean	.....	17·52	62·01	17·47	61·86
Heated to a bright red heat and cooled in oil at 100° C.	71.	19·322	70·390	19·678	71·687
	72.	19·594	70·864	19·993	72·307
	73.	19·826	72·358	19·975	72·901
	74.	19·805	72·281	19·845	72·427
	75.	19·361	71·103	19·401	71·317
Mean	.....	19·58	71·40	19·78	72·13

Table (*continued*).

Description of temper.	No. of magnet.	First magnetization.		Second magnetization.	
		Total magnetic moment.	Magnetic moment per gramme mass.	Total magnetic moment.	Magnetic moment per gramme mass.
Heated to a bright red heat and cooled in oil at 150° C.	76.	21·863	76·178	22·096	76·990
	77.	21·687	75·564	22·208	77·380
	78.	20·404	71·094	21·096	73·505
	79.	21·762	75·826	22·263	77·573
	80.	21·484	74·857	22·300	77·700
Mean	.....	21·44	74·71	21·99	76·63
Heated to a bright red heat and cooled in oil at 200° C.	81.	21·222	74·333	21·349	74·778
	82.	20·895	73·445	21·279	74·796
	83.	20·924	73·547	21·185	74·464
	84.	21·147	73·943	21·222	74·203
	85.	20·962	73·810	20·924	73·676
Mean	.....	21·03	73·82	21·19	74·38
Heated to a bright red heat and cooled in oil at 250° C.	86.	20·869	72·970	20·831	72·836
	87.	20·531	71·988	20·831	73·040
	88.	20·421	71·778	20·570	72·302
	89.	20·719	72·444	20·905	72·094
	90.	20·745	72·282	20·905	72·840
Mean	.....	20·66	72·29	20·81	72·62
Heated to a bright red heat and cooled in oil at 300° C.	91.	20·887	73·415	20·905	73·480
	92.	21·129	74·323	21·259	74·195
	93.	20·794	73·218	21·110	74·296
	94.	20·745	72·156	20·886	72·647
	95.	21·092	73·109	21·109	73·168
Mean	.....	20·93	73·24	21·05	73·56

XLV. *On Multiplication by a Table of Single Entry.* *By* J. W. L. Glaisher, *M.A., F.R.S.**

§ 1. IN the British-Association Report (Bradford, 1873, pp. 22, 23) I made the following remarks in reference to multiplication by means of tables of quarter squares:—

"In 1854, Professor Sylvester, having seen a paper in Gergonne in which the method was referred to, and not being aware that tables of quarter squares for facilitating multipli-

* Communicated by the Author.

cations had been published, suggested the calculation of such tables in two papers—'Note on a Formula by aid of which, and of a table of Single Entry, the continued product of any set of Numbers . . . . may be effected by additions and subtractions only, without the use of Logarithms' (Philosophical Magazine, [IV.] vol. vii. p. 430), and 'On Multiplication by aid of a Table of Single Entry' (Assurance Magazine, vol. iv. p. 236). Both these papers were probably written together; but there is added to the former a postscript, in which reference is made to Voisin and to Shortrede's manuscript. Professor Sylvester gives a generalization of the formula for ab as the difference of two squares, in which the product $a_1a_2 \ldots a_n$ is expressed as the sum of nth powers of $a_1, a_2, \ldots a_n$, connected by additive or subtractive signs. For the product of three quantities the formula is

$$abc = \tfrac{1}{24}\{(a+b+c)^3-(a+b-c)^3-(c+a-b)^3-(b+c-a)^3\}.$$

And at the end of the 'Philosophical-Magazine' paper Professor Sylvester has added some remarks on how a Table to give triple products should be arranged.

"At the end of a memoir, "Sur divers points d'Analyse," Laplace has given a section, "Sur la Réduction des Fonctions en Tables" (*Journal de l'École Polytechnique*, cah. xv. t. viii. pp. 258–265, 1809), in which he has briefly discussed the question of multiplication by a table of single entry. His analysis leads him to the method of logarithms, quarter squares, and also to the formula

$$\sin a \sin b = \tfrac{1}{2}\{\cos(a-b) - \cos(a+b)\},$$

by which multiplication can be performed by means of a table of sines and cosines. On this he remarks:—'Cette manière ingénieuse de faire servir des tables de sinus à la multiplication des nombres, fut imaginée et employée un siècle environ avant l'invention des logarithmes.'

"It is worth notice that the quarter-square formula is deduced at once from

$$\sin a \sin b = \tfrac{1}{2}\{\cos(a-b) - \cos(a+b)\},$$

by expanding the trigonometrical functions and equating the terms of two dimensions; similarly from

$$\sin a \sin b \sin c = \tfrac{1}{4}\{\sin(a+c-b) + \sin(a+b-c) + \sin(b+c-a) - \sin(a+b+c)\},$$

by equating the terms of three dimensions we obtain

$$abc = \tfrac{1}{24}\{(a+b+c)^3 - \&c.\},$$

as written down above, and so on, the general law being easily seen. We may remark that there is an important distinction between the trigonometrical formulæ and the algebraical deductions from them, viz. that by the latter to multiply two factors we require a table of squares, to multiply three a table of cubes, and so on; *i. e.* each different number of factors requires a separate table, while one and the same table of sines and cosines will serve to multiply any number of factors. This latter property is shared by tables of logarithms of numbers, the use of which is of course in every way preferable; still it is interesting to note the inferiority that theoretically attaches to the algebraical compared with the trigonometrical formulæ."

The object of this paper is to enter in some detail into the matters briefly referred to in the above extract.

§ 2. The method of quarter squares depends upon the formula

$$ab = \tfrac{1}{4}(a+b)^2 - \tfrac{1}{4}(a-b)^2;$$

so that, with the aid of a table of quarter squares, in order to multiply two numbers it is only necessary to enter the table with their sum and difference as arguments and take the difference of the tabular results. The first table of quarter squares was published by Voisin at Paris in 1817, and extends to 20,000; and the largest that has appeared was published by the late Mr. S. L. Laundy, and extends to 100,000. General Shortrede's manuscript table, that extended to 200,000, and so would enable five figures to be multiplied by five figures, has not been printed. Ludolf, who in 1690 published a table of squares to 100,000, explains in his introduction how it can be applied to effect multiplications by means of the above formula. The title of Voisin's work is *Tables des multiplications, ou logarithmes des nombres entiers depuis* 1 *jusqu'à* 20,000, *au moyen desquelles on peut multiplier tous les nombres qui n'excèdent pas* 20,000 *par* 20,000, *et généralement faire toutes les multiplications dont le produit n'excède pas* 400,000,000 . . . , *par Antoine Voisin* . . . . By a logarithm is here meant a quarter square, viz. Voisin calls a a *root*, and $\frac{1}{4}a^2$ its *logarithm*. If the sum of the two numbers to be multiplied exceeds the limits of the table, but each of the numbers is included in it, the multiplication is to be effected by means of the formula

$$ab = 2\{\tfrac{1}{4}a^2 + \tfrac{1}{4}b^2 - \tfrac{1}{4}(a-b)^2\}.$$

Voisin is thus justified in stating that by means of his table, any two numbers neither of which exceeds 20,000 may be multiplied together; but it is clear that if the sum of the fac-

tors exceeds 20,000 the method loses its advantages, as the last-written formula requires three entries and a duplication.

An ordinary multiplication table, or Pythagorean table, giving the product ab for arguments a, b is of double entry, and so could not be carried to any very considerable extent on account of the great bulk of the table. Herwart ab Hohenburg's table of 1610, referred to in §§ 9–12, extends to 1000×1000, as also is the case with Crelle's *Rechentafeln*, which are in general use; but the Pythagorean table has never been carried beyond this limit. The question of the reduction of the process of multiplication to that of addition or subtraction is one that is interesting both from a practical and historical point of view.

§ 3. In the Philosophical Magazine, [IV.] vol. vii. pp. 431, 432 (1854), Sylvester gave the generalization of the quarter-square formula for the product of n quantities in the following form:—

"Let $\theta_1, \theta_2, \theta_3, \ldots \theta_n$ be disjunctively equal to 1, 2, 3, $\ldots n$; then

$$\begin{aligned}(2\,.\,4\,.\,6\ldots 2n)(a_1\,a_2\ldots a_n)\\ =(a_{\theta_1}+a_{\theta_2}+a_{\theta_3}+\ldots+a_{\theta_n})^n-\Sigma(-a_{\theta_1}+a_{\theta_2}+\ldots+a_{\theta_n})^n\\ +\Sigma(-a_{\theta_1}-a_{\theta_2}+a_{\theta_3}+\ldots+a_{\theta_n})^n+\ldots\\ +(-)^n(-a_{\theta_1}-a_{\theta_2}-\ldots-a_{\theta_n})^n.\text{"} \quad . \ . \ . \ . \ . \ . \quad (1)\end{aligned}$$

The first and last terms, the second and last but one, &c., are identical and may be united, there being one term left over in the middle if n be even; viz. this becomes

$$\begin{aligned}(4\,.\,6\,.\,8\ldots 2n)(a_1\,a_2\ldots a_n)=(a_{\theta_1}+a_{\theta_2}+a_{\theta_3}\ldots+a_{\theta_n})^n\\ -\Sigma(-a_{\theta_1}+a_{\theta_2}+a_{\theta_3}\ldots+a_{\theta_n})^n\\ +\text{&c.}; \quad . \ . \ . \ . \ . \ . \ . \quad (2)\end{aligned}$$

the last term being

$$(-)^m\Sigma(-a_{\theta_1}-a_{\theta_2}\ldots-a_{\theta m}+a_{\theta m+1}+a_{\theta m+2}\ldots+a_{\theta_n})^n$$

if $n=2m+1$, and

$$\tfrac{1}{2}(-)^m\Sigma(-a_{\theta_1}-a_{\theta_2}\ldots-a_{\theta m}+a_{\theta m+1}+a_{\theta m+2}\ldots+a_{\theta_n})^n$$

if $n=2m$. This last expression is integral, notwithstanding the factor $\frac{1}{2}$, since each term composing it occurs twice; as, *ex. gr.*, when $n=2$, the whole term is $\frac{1}{2}\{(a-b)^2+(b-a)^2\}$. These are the formulæ given by Sylvester on p. 432. The equation (1) is there proved by showing that the expression on the right-hand side vanishes when $a_n=0$, and therefore when $a_1=0$, $a_2=0$, &c., so that it $=k a_1\,a_2\ldots a_n$, and determining the numerical factor k by putting $a_1=a_2=\ldots=a_n=1$.

§ 4. The formulæ can, however, be proved in the manner

indicated in § 1, viz. by equating the terms of the nth order in the equation giving $\sin a_1 \sin a_2 \ldots \sin a_n$ as a sum of sines or cosines.

Denoting by $\Sigma_r \sin a$ the sum of the sines of the angles $\pm a_1 \pm a_2 \ldots \pm a_n$, in which r signs are negative and $n-r$ positive, viz. denoting by $\Sigma_r \sin a$ the expression which would be written in Sylvester's notation,

$$\Sigma \sin(-a_{\theta_1} - a_{\theta_2} \ldots - a_{\theta_r} + a_{\theta r+1} + a_{\theta r+2} \ldots + a_{\theta_n}),$$

and attaching a similar meaning to $\Sigma_r \cos a$, we have

$$(-)^m 2^{n-1} \sin a_1 \sin a_2 \ldots \sin a_n = \sin(a_1 + a_2 \ldots + a_n) - \Sigma_1 \sin a + \Sigma_2 \sin a \ldots + (-)^m \Sigma_m \sin a$$

if $n = 2m+1$, and

$$(-)^m 2^{n-1} \sin a_1 \sin a_2 \ldots \sin a_n = \cos(a_1 + a_2 \ldots + a_n) - \Sigma_1 \cos a + \Sigma_2 \cos a \ldots + \tfrac{1}{2}(-)^m \Sigma_m \cos a$$

if $n = 2m$. The factor $\frac{1}{2}$ has here the same explanation as before; viz. each term under the sign Σ_m occurs twice over, and the $\frac{1}{2}$ merely causes it to be counted once instead of twice. The truth of these formulæ is readily seen by starting with $\sin a \sin b = \frac{1}{2}\{\cos(a-b) - \cos(a+b)\}$, multiplying by $\sin c$ and obtaining the expression as a sum of sines, then multiplying by $\sin d$ and so on; the general law then soon becomes apparent. There is a simple and direct investigation of the formulæ by Mr. R. Verdon in the 'Messenger of Mathematics,' vol. vii. pp. 122–124 (1877).

The formulæ of § 3 in the form (2) follow at once by equating terms of the nth order in the expansion of the terms in these trigonometrical formulæ, or, what is the same thing, by writing $a_1 x, \ldots a_n x$ for $a_1, \ldots a_n$ and equating coefficients of x^n.

We also see that if in the formulæ in § 3, the exponent, instead of being equal to n, the number of quantities $a_1, \ldots a_n$, be less than n, and differ from it by an even number, the expressions on the right-hand side of the equations are equal to zero, and that in general, if we denote

$$\Sigma(-a_{\theta_1} \ldots - a_{\theta_r} + a_{\theta_{r+1}} \ldots + a_{\theta_n})^p$$

by $\Sigma_r a^p$, then

$$(a_1 + a_2 \ldots + a_n)^p - \Sigma_1 a^p + \Sigma_2 a^p - \&c., \quad . \quad . \quad (3)$$

the last term being $(-)^m \Sigma_m a^p$ if $n = 2m+1$, and $\frac{1}{2}(-)^m \Sigma_m a^p$ if $n = 2m$, is equal to $\pm 2^{n-1} p!\, a_1 a_2 \ldots a_n \times$ the terms of the $(p-n)$th order in

$$\left(1 - \frac{a_1^2}{3!} + \frac{a_1^4}{5!} - \&c.\right)\left(1 - \frac{a_2^2}{3!} + \&c.\right) \ldots \left(1 - \frac{a_n^2}{3!} + \&c.\right),$$

where $p!$ denotes $1.2.3\ldots p$, and p, n are supposed to be both even or both uneven. It is easily seen, by considering separately the two cases of p even and p uneven, that the terms in the resulting expression are always positive; so that (3) is equal to $2^{n-1}p!\ a_1 a_2 \ldots a_n \times$ the terms of the $(p-n)$th order in

$$\left(1+\frac{a_1^2}{3!}+\frac{a_1^4}{5!}+\&c.\right)\left(1+\frac{a_2^2}{3!}+\&c.\right)\ldots\left(1+\frac{a_n^2}{3!}+\&c.\right)^*.$$

Thus, for example, let $n=3$; then

$$(a+b+c)\ -(b+c-a)\ -(c+a-b)\ -(a+b-c)\ =0,$$

$$(a+b+c)^3-(b+c-a)^3-(c+a-b)^3-(a+b-c)^3=24abc,$$

$$(a+b+c)^5-(b+c-a)^5-(c+a-b)^5-(a+b-c)^5$$
$$=80abc(a^2+b^2+c^2),$$

$$(a+b+c)^7-(b+c-a)^7-(c+a-b)^7-(a+b-c)^7$$
$$=2^2.7!\,abc\left(\frac{a^4+b^4+c^4}{5!}+\frac{b^2c^2+c^2a^2+a^2b^2}{(3!)^2}\right)$$
$$=56abc(3a^4+3b^4+3c^4+10b^2c^2+10c^2a^2+10a^2b^2)$$

&c. &c.

Let $n=4$, then

$$(a+b+c+d)^p$$
$$-(-a+b+c+d)^p-(a-b+c+d)^p-(a+b-c+d)^p$$
$$-(a+b+c-d)^p$$
$$+(-a-b+c+d)^p+(a-b-c+d)^p+(a+b-c-d)^p$$
$$=0 \text{ if } p=2,$$
$$=192abcd \text{ if } p=4,$$
$$=960abcd(a^2+b^2+c^2+d^2) \text{ if } p=6,$$
$$=896abcd(3a^4+3b^4+3c^4+3d^4+10a^2b^2+10a^2c^2+10a^2d^2$$
$$+10b^2c^2+10b^2d^2+10c^2d^2) \text{ if } p=8, \&c.$$

§ 5. We can also obtain an expression for the sum of powers when the exponent p is even and all the terms have the positive sign. For

$$2^{n-1}\cos a_1\cos a_2\ldots\cos a_n=\cos(a_1+a_2\ldots+a_n)$$
$$+\Sigma_1\cos a+\Sigma_2\cos a+\ldots,$$

---

* Since $\sin ix=i\sinh x$, $\cos ix=\cosh x$, the trigonometrical formulæ become, on writing $ia_1,\ldots ia_n$ for $a_1,\ldots a_n$:—

$$2^{n-1}\sinh a_1\sinh a_2\ldots\sinh a_n=\sinh(a_1+a_2\ldots+a_n)-\Sigma_1\sinh a+\&c.$$

if $n=2m+1$, and

$$=\cosh(a_1+a_2\ldots+a_n)-\Sigma_1\cosh a+\&c.$$

if $n=2m$, leading at once to the theorem in the above form.

the last term being $\Sigma_m \cos a$ if $n=2m+1$, and $\frac{1}{2}\Sigma_m \cos a$ if $n=2m$; and equating the terms of the $2q$th order, we find that

$$(a_1+a_2\ldots+a_n)^{2q}+\Sigma_1 a^{2q}+\Sigma_2 a^{2q}+\text{ &c.},$$

the last term being $\Sigma_m a^{2q}$ if $n=2m+1$, and $\frac{1}{2}\Sigma_m a^{2q}$ if $n=2m$, is equal to $2^{n-1}.\,2q!\times$ the terms of the $2q$th order in

$$\left(1+\frac{a_1^2}{2!}+\frac{a_1^4}{4!}+\text{&c.}\right)\left(1+\frac{a_2^2}{2!}+\text{&c.}\right)\ldots\left(1+\frac{a_n^2}{2!}+\text{&c.}\right).$$

Thus

$$(a+b+c)^4+(b+c-a)^4+(c+a-b)^4+(a+b-c)^4 \\ =4(a^4+b^4+c^4+6b^2c^2+6c^2a^2+6a^2b^2);$$

and in the case of n quantities $a_1, a_2, \ldots a_n$,

$$(a_1+a_2\ldots+a_n)^4+\Sigma_1 a^4+\Sigma_2 a^4+\text{ &c.} \\ =2^{n-1}(a_1^4\ldots+a_n^4+6a_1^2a_2^2\ldots+6a_{n-1}^2a_n^2),\ \text{&c.}$$

It may be remarked here, that any trigonometrical identity in which the arguments are homogeneous functions of the letters gives rise to a series of algebraical identities by equating the terms of each order; *ex. gr.* from

$$\sin(d-b)\sin(a-c)+\sin(b-c)\sin(a-d) \\ +\sin(c-d)\sin(a-b)=0,$$

we have, by equating terms of the fourth order,

$$(d-b)(a-c)\{(d-b)^2+(a-c)^2\} \\ +(b-c)(a-d)\{(b-c)^2+(a-d)^2\} \\ +(c-d)(a-b)\{(c-d)^2+(a-b)^2\}=0.$$

There are a great number of trigonometrical identities of this kind, such as

$$\sin(b-c)+\sin(c-a)+\sin(a-b) \\ +4\sin\tfrac{1}{2}(b-c)\sin\tfrac{1}{2}(c-a)\sin\tfrac{1}{2}(a-b)=0,$$

$$\cos(a+b)\sin(a-b)+\cos(b+c)\sin(b-c) \\ +\cos(c+d)\sin(c-d)+\cos(d+a)\sin(d-a)=0,\ \text{&c.};$$

and some of the algebraical identities thus obtained are of interest. An identity of this class is referred to in the 'Messenger of Mathematics,' vol. viii. p. 46 (July 1878).

§ 6. In Laplace's section, "Sur la Réduction des Fonctions en Tables," he considers the question of multiplication by means of a Table of single entry. First, assuming that $xy=\phi(X+Y)$, where X is a function of x only, and Y a

function of y only, it is found that

$$x = \mathrm{A}e^{q\mathrm{X}}, \quad y = \mathrm{B}e^{q\mathrm{Y}},$$

giving the method of logarithms; and assuming

$$xy = \phi(\mathrm{X}+\mathrm{Y}) - \phi(\mathrm{X}-\mathrm{Y}),$$

it is found that solutions are :—

$$\phi(\mathrm{X}+\mathrm{Y}) = \tfrac{1}{4}(x+y)^2;$$

and

$$x = \sin \mathrm{X}, \quad y = \sin \mathrm{Y}, \quad \phi(\mathrm{X}+\mathrm{Y}) = -\tfrac{1}{2}\cos(\mathrm{X}+\mathrm{Y}).$$

Laplace then shows that the values of $f(x, y)$ can be calculated by means of a table of single entry, if the differential equation obtained by eliminating c from the equation $f(x, y) = c$ be of the form $\mathrm{S}dx + \mathrm{T}dy = 0$, S being a function of x only, and T a function of y only.

It is clear that the formula

$$\sin a \sin b = \tfrac{1}{2}\{\cos(a-b) - \cos(a+b)\}$$

does reduce multiplication to addition or subtraction by means of a table of sines; and Laplace's remark that tables of sines had actually been used in this manner for about a century before the invention of logarithms led me to search for the history of this curious method.

§ 7. The method in question was called *prosthaphæresis*, often written in Greek letters προσθαφαίρεσις, and had its origin in the solution of spherical triangles. A careful examination of the history of the method is given by Scheibel in his *Einleitung zur mathematischen Bücherkenntniss* : *siebentes Stück* (Breslau, 1775), pp. 13–20; and there is also an account in Kästner's *Geschichte der Mathematik*, t. i. (1796) pp. 566–569, in Montucla's *Histoire des Mathématiques*, t. i. pp. 583–585 and 617–619, and in Klügel's *Wörterbuch* (1808), article *Prosthaphæresis*. The method consists in the use of the formula

$$\sin a \sin b = \tfrac{1}{2}\{\cos(a-b) - \cos(a+b)\},$$

by means of which the multiplication of two sines is reduced to the addition or subtraction of two tabular results taken from a table of sines; and as such products occur in the solution of spherical triangles, the method affords the solution of spherical triangles in certain cases by addition and subtraction only. It seems to be due to Wittich, of Breslau, who was assistant for a short time to Tycho Brahe*; and it was used by

* Christmann, in his *Theoria Lunæ*, states that the first inventor of the method was Werner of Nuremberg, who employed it in a treatise *De Triangulis*, which was never printed (Montucla, t. i. p. 584).

them in their calculations in 1582. Wittich in 1584 made known at Cassel the calculation of one case by this prosthaphæresis; and Justus Byrgius proved it in such a manner that from his proof the extension to the solution of all triangles could be deduced. Clavius generalized the method in his treatise *De Astrolabio* (1593), lib. i. lemma liii. The lemma commences as follows:—

"*Quæstiones omnes, quæ per sinus, tangentes, atque secantes absolvi solent, per solam prosthaphæresim, id est, per solam additionem, subtractionem, sine laboriosa numerorum multiplicatione, divisioneque expedire.*

"Edidit ante tres, quatuorve annos Nicolaus Raymarus Ursus Dithmarsus libellum quendam, in quo præter alia proponit inventum sane acutum, et ingeniosum, quo per solam prosthaphæresim pleraque triangula sphærica solvit. Sed quoniam id solum putat fieri posse, quando sinus in regula proportionum assumuntur, et sinus totus primum locum obtinet, conabimur nos eam doctrinam magis generalem efficere, ita ut non solum locum habeat in sinibus, et quando sinus totus primum locum in regula proportionum obtinet, verum etiam in tangentibus, secantibus, sinibus versis et aliis numeris, et sive sinus totus sit in principio regulæ proportionum, sive in medio, sive denique nullo modo interveniat: quæ res nova omnino est, ac jucunditatis et voluptatis plena."

The work of Raymarus Ursus, referred to by Clavius, is his *Fundamentum Astronomicum* (1588). Longomontanus, who also assisted Tycho Brahe, in his *Astronomia Danica* (1622) gives an account of the method, stating that it is not to be found in the writings of the Arabs or Regiomontanus. Scheibel also mentions a manuscript "Melchior Jöstelii logistica *προσθαφαίρεσις* astronomica" (1609).

With the exception of Clavius, I have not examined the works referred to, but have relied on Scheibel and the other writers mentioned at the beginning of this section. It did not seem necessary to enter further into the matter, as there can be no doubt that the method of prosthaphæresis is that to which Laplace's remark refers, and that it was used for performing multiplications, even when the quantities to be multiplied did not present themselves as sines and cosines. It need scarcely be remarked that when the method was in use (previous to the invention of logarithms) the cosine had not been introduced; so that the rules for the different cases of $\sin a \sin b$, $\sin a \cos b$, &c. were complicated, it being necessary to frequently pass from the angles to their complements. In Klügel's *Wörterbuch* it is mentioned that, if other trigonometrical functions have to be multiplied besides sines and

cosines, or if a sine or cosine is a divisor, the process is more troublesome—that these multiplications can be effected by the formula, but that the requisite transformations are more laborious than the multiplication itself, which is purely mechanical, while with the prosthaphæresis method more care is required and, in addition, it is more difficult to obtain accuracy in the result. These remarks seem to be obviously just; and it is clear that the method could not be a good one for the ordinary multiplication of numbers not given in the form of sines of angles, as four entries of the tables would be necessary, besides very troublesome interpolations.

§ 8. Napier published his *Canon Mirificus* in 1614; and then the prosthaphæresis method was at once superseded by logarithms. The latter process requires only three entries of the table in order to multiply two numbers, and, even regarded merely as a multiplication method, is greatly superior in every respect to that of prosthaphæresis, which requires four entries. Before the invention of logarithms the object was to arrange formulæ, of which numerical values were required, as sums of sines by prosthaphæresis, so that they might admit of calculation by addition or subtraction; since the invention of logarithms the object has always been to throw formulæ into the form of products.

Regarded as processes for effecting multiplications, the methods of (1) prosthaphæresis, (2) logarithms, and (3) quarter squares, may be compared as follows:—The first theoretically solves the question, but is impracticable as a general method. The second is the best method, if only a few figures (viz. 6 or at the most 9 figures) of the product are wanted: if n numbers are to be multiplied together, only $n+1$ entries are required. The method of quarter squares is the best if only two numbers are to be multiplied together, and if all the figures of the product are wanted. Only two entries are required; and a table from 1 to 200,000 (which would only occupy a moderate octavo volume) would give the product of any two five-figure numbers by two entries and one subtraction, no interpolations being necessary. A Pythagorean table of this extent would be absolutely impossible, as it would occupy 100×100, or 10,000 volumes similar to Crelle's *Rechentafeln*, which in the new edition occupies one volume folio (see § 10). The quarter-square method seems not to have been much used, partly because it has never become generally known, and partly because no table exceeding 100,000, and therefore available for all five-figure numbers, has been published. Such tables are also only suitable for the one purpose of multiplication, while logarithms have a great variety of uses.

The connexion between prosthaphæresis and quarter squares, and between the generalization of the prosthaphæresis formula, as given in § 4, and the formula for the product of n quantities as a sum of nth powers, viz. that the algebraical formulæ may be obtained by equating terms of the same order in the expansions of the trigonometrical formula, is noteworthy; but, as mentioned in § 1, the trigonometrical formula only requires one and the same table (a table of sines), however many quantities have to be multiplied together, while the algebraical formula requires a table of squares to multiply two numbers, a table of cubes to multiply three, and a table of nth powers to multiply n numbers; and to multiply n numbers, 2^{n-1} entries would be required. We might, however, multiply two numbers by means of a table of quarter squares, and then multiply their product by the third number and so on (which would also require 2^{n-1} entries); but we should only obtain the exact value of the result if all the products were included within the limits of the arguments of table, *i. e.* if the sum of the largest multiplier and the product of the other factors be within these limits. Practically, however, the sine table would only contain a certain number of decimal places; and if we assume the sine table to be perfect so as to render any number of multiplications possible, we ought at the same time to assume the quarter-square table to be extended *ad libitum*; so that the theoretical advantage of the trigonometrical formula, as only requiring one table, is more apparent than real, if we admit the repeated use of the quarter-square table.

As just remarked, a table of sines, like a table of logarithms, is only available for obtaining results to a certain number of figures; and the superiority of the quarter-square method, when only numbers within the range of the table have to be multiplied together, is very decided.

§ 9. The title of Herwart ab Hohenburg's multiplication table is "Tabulæ arithmeticæ προσθαφαιρεσεως universales, quarum subsidio numerus quilibet, ex multiplicatione producendus, per solam additionem; et quotiens quilibet, e divisione eliciendus, per solam subtractionem, sine tædiosâ & lubricâ Multiplicationis, atque Divisionis operatione, etiam ab eo, qui Arithmetices non admodum sit gnarus, exactè, celeriter & nullo negotio invenitur. È museo Ioannis Georgii Herwart ab Hohenburg . . . . Monachii Bavariarum. Anno Christi, M.DC.X." The book is a very large and thick folio, containing a multiplication table up to 1000×1000, the thousand multiples of any one number being given on the same page; and there is an introduction of seven pages, in which the use of the table in multiplying numbers containing more than three

figures, and in the solution of spherical triangles, is explained.

The mathematical bibliographers and historians afford very little information with regard to the work. Heilbronner (*Historia Matheseos*, 1742, p. 801) gives the title not quite correctly, and adds, "Docet in his tabulis sine abaco multiplicationem atque divisionem perficere." Kästner (*Geschichte der Mathematik*, 1796–1800, t. iii. p. 8) quotes the title from Heilbronner and his remark, and adds that Heilbronner could not have known Herwart's method, or he would have described it. He remembers to have read somewhere that the book contained a number of tables of products, arranged by factors, like a great multiplication table. Scheibel (*Einleitung zur mathematischen Bücherkenntniss : eilftes Stück*, 1779, pp. 417–420) gives the full title of the work, a description of it, and an example worked out to show how it is to be applied to multiply numbers of more than three figures. He commences his account:—"Der Herausgeber dieses colossalischen Einmal Eins ist also kein anderer, als der so berühmte Staatsmann und Geschichtschreiber, dessen sehr wichtiges und seltenes Werk : *Ludovicus* IV *Imperator defensus* zu München 1618. 1619. in gr. 4. in der Hamburg. Histor. Bibliothek Cent. VIII. Art. 97, beschrieben wird;" and concludes, "So viel von diesem ungeheuren Folianten, den man bloss zur Curiosität und seiner Seltenheit wegen, in einer mathematischen Büchersammlung aufbewahret." Montucla (*Histoire des Mathématiques*, t. ii. pp. 13, 14) gives a short account of the table and the mode of using it when the numbers to be multiplied contain more than three figures. This is introduced by the words:—"Nous terminons cet article, en faisant connoître un livre assez obscur, dont l'objet étoit d'abréger les calculs arithmétiques, et qui n'auroit pas été inutile, sans l'invention des logarithmes;" and Montucla adds further on, "Tel est l'esprit de cette invention, qui, sans la decouverte des logarithmes, auroit pu être de quelque utilité aux calculateurs, si toutefois la peine de chercher ces produits, au nombre de 6, 7, 8 ou 9, dispersés dans un énorme *in-folio*, n'eût pas paru plus fatigante et non moins laborieuse que le calcul même, pour un homme exercé." Murhard (*Bibl. Math.* 1797–1804, t. ii. p. 199) gives the title correctly, and marks it with an asterisk to show that he has seen the book itself. Rogg (*Bibl. Math.* 1830, p. 142) merely has, "Hohenburg, Gregor (*sic*) Herwardt ab, tabulæ arithmeticæ προσθαφαιρεσεως universales, 1610." Neither Weidler (*Bibl. Ast.* 1755), Deschales (*Cur. seu Mund. Math.* 1690), Lalande (*Bibl. Ast.* 1803), nor Delambre (*Hist. de l'Ast. Mod.* 1821) mention the work; but there is a refer-

ence to it in Leslie's 'Philosophy of Arithmetic' (2nd edit. 1820, p. 246). In his article on Tables in the English Cyclopædia (1863), De Morgan, referring to this table, wrote :— "The table goes up to 1000×1000, each page taking one multiplier complete. There are then a thousand odd pages; and as the paper is thick, the folio is almost unique in thickness. There is a short preface of seven pages, containing examples of application to spherical triangles. It is truly remarkable that while the difficulties of numerical calculation were stimulating the invention of logarithms, they were giving rise to this the earliest work of extended tabulated multiplication. Herwart passes for the author; but nothing indicates more than that the manuscript was found in his possession. The book is excessively rare; a copy sold by auction a few years ago was the only one we ever saw." Graesse (*Trésor des livres rares*, 1859–1867) says that by the book the use of logarithms was first spread in Germany, which is obviously erroneous.

§ 10. Crelle's *Rechentafeln*, which also extend to 1000×1000, were first published in two octavo volumes in 1820. A second (stereotype) edition in one volume (folio) was published under the editorship of the late Dr. Bremiker in 1864. This work is well known, and is much used. From Crelle's preface to his table in 1820, it is clear that he knew nothing of Herwart's work, and was not aware that a table to 1000×1000 had ever been published; for he writes, "Das neueste und vielleicht bedeutendste Unternehmen von Tafeln, die mit den gegenwärtigen einerlei Idee zum Grunde haben, ist das Werk : 'Tables de multiplication, à l'usage de MM. les géomètres, de MM. les ingénieurs vérificateurs du Cadastre etc. Sec. édit., Paris, chez Valace, 1812;'" and then he states that this work only extends to 500×500, and is therefore the fourth part of his own table. It is not a little remarkable that the first multiplication table of any extent published should have reached the limit beyond which the table has never been carried. That Herwart's idea was good and practicable is proved by the continual use made of Crelle's tables; and but for the great bulk of his ponderous volume, the table would in all probability have come into use. The invention of logarithms four years afterwards afforded another means of performing multiplications, and Herwart's huge work never became generally known. It is curious that from 1610 till 1820 no similar table should have been published, as it seems clear that the work of 1610 was a failure chiefly in consequence of its inconvenient size. Herwart's table is rare; but there are copies in the British Museum, the Bodleian Library, and the Graves

Library at University College, London; the last is not quite perfect.

§ 11. While I was engaged in preparing the British-Association report, I endeavoured without success to find any thing relating to the history of the table; but the hope is there expressed that, considering the attention so large a work must have received from contemporary mathematicians, some information might still be gained with regard to the calculator of the tables, his objects, &c. I afterwards met with a correspondence of six letters between Herwart and Kepler, which took place at the end of 1608, and throws light upon these points. The letters are printed in Dr. Frisch's *Joannis Kepleri Astronomi opera omnia*, t. iv. pp. 527–530, 1863. Herwart, who was Chancellor of the Palatinate of Bavaria, and a man of mark in his time as a statesman, was a frequent correspondent of Kepler's; and many of his letters upon chronology are printed in Kepler's *Eclogæ Chronicæ*. There is a gap in the correspondence, however, between January 12, 1608, and December 5, 1608; and Dr. Frisch, in the notes to the *Eclogæ*, gives the six letters referred to, prefaced by the words "Keplerus in Eclogis omisit epistolas Herwarti datas d. 13 Sept. et 5 Nov. 1608, ipsiusque responsionem d. d. 18 Oct., quum nihil facerent ad Chronologiam, et maxima ex parte spectarent opus Herwarti arithmeticum, quod edidit Monachii 1610..." In the first letter, dated September 13, 1608, Herwart writes:—"Ich hab bisher in Multiplicatione et Divisione sonderbare geschriebene praxin gebraucht, dadurch ich den numerum ex quavis multiplicatione productum, per solam additionem, und den Quotienten ex divisione resultantem per solam subtractionem (absque tædiosa multiplicationum et divisionum operatione) gefunden." He states that J. Prætorius and others who have seen it, recommend him to have it printed; and he adds that if he had not had this method (*diesen modum*), on account of his continual occupations, and because he is not a good calculator, he should long ago have had to give up all mathematical matters requiring calculation. He sends a specimen page of the Table, the use of which he illustrates by the multiplication $945{,}678 \times 587$, and he asks Kepler to give him his opinion upon the subject.

Kepler replies on October 18, 1608, and remarks that tables are very useful to the sedentary man, and "ei, qui perpetuo cum libris cohabitat." For 500 or 1000 pages form a large volume, which cannot always be at hand. As it will facilitate astronomical calculations he advises that short precepts on the solution of triangles should be added. Kepler then proceeds:—"Nam multis partibus expeditius est uti hoc tuo

volumine, quam προσθαφαιρεσει Witichiana, quæ sæpissime inducit in tabulam sinuum, crebro permutat sinus complementis, arcus rectis, rectas arcubus. Illaque memoriter nunquam retineo, et tædiosum est, toties adire scripta præcepta, præsertim ingenioso, qui nihil sine causæ cognitione cupit agere. Requirit præterea cautiones crebras, ex quibus neglectis crebra offendicula. Causæ operationum etsi ingeniosissimæ tamen semper in abstruso sunt inter operandum. At si multiplicemus et dividamus simpliciter, tunc videmus quid agamus; et possunt varietates triangulorum talibus præceptis comprehendi, quæ memoria retineri facile possunt." Then follows a synopsis of the different cases of the solution of spherical right-angled triangles. Herwart writes on November 5, and says he had found that triangles could be solved better and more quickly by means of his table than by prosthaphæresis, so that Kepler was quite right. He proceeds :— "Darunter ich aber Vitichium nit gesehen. Dieser abacus ist universalis. Weil man pflegt den Büchern ein splendidum titulum zu geben, ut reddantur venales, wollt ich gern dessen Gutachten vernehmen, ob es nit thunlich ungefähr so : Nova exacta, certa et omnium facillima ratio Arithmetices, per quam numerus ex multiplicatione productus sine operatione multiplicationis per solam additionem, et quotiens ex divisione resultans absque operosis ambagibus divisionis per solam sub tractionem cujuscunque, etiam maximæ summæ, etiam ab eo, qui arithmetices non admodum sit gnarus, citius quam ulla alia ratione invenitur." He then asks Kepler for his advice as to how the book should be entitled in Latin and German, and prays him to write soon.

Not receiving an answer, on December 2 he sends a short letter to Kepler, again asking for a reply, and suggesting that perhaps it was not prudent "so speciosum titulum tantillæ rei zu præfigiren." But immediately after, Kepler's reply [which is lost] was received; and writing on December 5, Herwart explains that he does not expect an answer to his last letter, and that he understands that Kepler has no suggestion to make with regard to the title, but thinks it should be shortened. He cannot understand the meaning of Kepler's advice, "Græca compositio imploranda, sed exercito," and asks for an explanation.

In the last letter of the correspondence, dated December 12, 1608, Kepler explains that, as the title seemed long, he had advised that it should be shortened by the composition of two Greek words, as in ἐρυσιπελας, ῥιψοκινδυνος, &c., and continues:—"Mihi quidem cum non occurreret quidquam hujusmodi, quod ad nostram rem faceret, consului opem imploran-

dam alicujus exercitati in Græca lingua. Quid si occurrerit quod placet? Σεισαχθεια ἀριθμητικη. Nosti enim, χρεων ἀποκοπας sic dici. Inest vocabulo et emphasis et proprietas et similitudinis gratia, quia me Hercule *novas tabulas* introducis, et uno ictu liberas computatores debitis multiplicandi et dividendi inextricabilibus. Sed facile est exercitato, copiam afferre similium compositorum, ut ex iis aptius aliquod eligatur. Mihi jam plura non occurrunt." He then refers to the *Tabula Tetragonica* [Venice, 1592] of Maginus, and adds in a postscript "Titulus igitur talis: Σεισαχθεια sive Novæ Tabulæ, quibus Arithmetici debitis inextricabilibus multiplicandi et dividendi liberantur, ingenio, tempori viribusque ratiocinantis consulitur."

§ 12. It thus appears that the table was printed from a manuscript that Herwart used himself, and which very likely he had had made. As for the word prosthaphæresis which occurs on the title, it is shown by the correspondence that Herwart used the table for multiplications in general, and that it was Kepler who pointed out that by means of it spherical triangles could be solved more easily than by the prosthaphæresis of Wittich, and suggested the addition of the precepts for the solution of triangles, which actually occur in the preface to the work. The title suggested by Kepler was not adopted, nor was his advice about shortening it; but it must be acknowledged that Herwart succeeded in obtaining a "splendid title," which also contained a Greek word. De Morgan explained the use of the word "prosthaphæresis" upon the title-page of Herwart's table, thus: "Prosthaphæresis is a word compounded of prosthesis and aphæresis, and means addition and subtraction. Astronomical corrections, sometimes additive and sometimes subtractive, were called prosthaphæreses. The constant necessity for multiplication in forming proportional parts for the corrections, gave rise to this table, which therefore had the name of its application on the title-page." The correspondence, however, shows that the table derived its title not from its general use in the calculation of astronomical prosthaphæreses, but from the special prosthaphæresis of Wittich for the solution of triangles. In a paper on Herwart's Table read before the Cambridge Philosophical Society* on October 25, 1875, which contained the greater part of the contents of §§ 9 and 11 of the present paper, I remarked that the prosthaphæresis referred to seemed to be most likely a method of solving spherical triangles, in which the product of

* "On Herwart ab Hohenburg's *Tabulæ Arithmeticæ προσθαφαιρεσεως universales*, Munich, 1610." Proceedings of the Cambridge Philosophical Society, vol. ii. pp. 386–392 (part xvi.).

two sines, or a sine and cosine, was avoided by the use of a formula such as $\sin a \sin b = \frac{1}{2}\{\cos(a-b) - \cos(a+b)\}$, adding that Laplace referred to such a method. This inference is shown to have been correct by the contents of § 7. Wittich does not appear to have published the method himself, though from the writings mentioned in § 7, and from Kepler's letters, it is clear that it was generally attributed to him: he ought, I suppose, to be considered the discoverer of the formulæ $\sin a \sin b = \frac{1}{2}\{\cos(a-b) - \cos(a+b)\}$, &c., which are really the prosthaphæretical formulæ. Kepler's remarks upon the difficulty of using the prosthaphæresis for spherical angles, on account of the confusion between sides and angles and their complements, is interesting; and it is for this reason that I have quoted so much of the letter of October 18. The word prosthaphæresis often means the difference between the true and mean places of a body in longitude or latitude; but it seems to have been vaguely used, very much as "correction" is now, to denote small quantities to be added or subtracted to quantities obtained by theory, or by a first approximation, &c.; so that without a context its signification is not precise; but I have not examined this point. In Klügel the word is derived from *πρόσθεν* and *ἀφαίρεσις*; but, at all events, as far as the mathematical and astronomical use of the word is concerned, De Morgan's derivation from *πρόσθεσις* and *ἀφαίρεσις* seems to be certainly the true one.

As it happened, Herwart's employment of the word *προσθαφαιρεσεως* upon his title-page was not fortunate; for only four years after the publication of his table logarithms were invented, all the processes of calculation were changed, and Wittich's prosthaphæresis passed out of notice.

Kepler, as is well known, greatly admired Napier's invention, and in 1624 published himself a table of Napierian logarithms.

It will have been noticed that Herwart describes his table as enabling multiplications to be performed "per solam additionem," and division "per solam subtractionem." These words would immediately suggest to a writer of the last or present century the method of logarithms; and it is for this reason, no doubt, that not unfrequently the methods of prosthaphæresis and quarter squares have been confounded with applications of logarithms. Voisin, as mentioned in § 2, actually called his quarter squares logarithms; and this has added to the confusion.

Trinity College, Cambridge.
July 12th, 1878.

XLVI. *Magnetic Figures illustrating Electrodynamic Relations.* *By* Silvanus P. Thompson, *D.Sc., B.A., F.R.A.S., Professor of Experimental Physics in University College, Bristol**.

[Plates VI. and VII.]

In a preliminary communication to the Physical Society in February of the present year, the author announced a method of studying and illustrating the known laws of the mutual attractions or repulsions of conductors traversed by electric currents. The present paper is a complete statement of the facts obtained in the experimental research which formed the basis of that communication.

While preparing a set of magnetic currents to illustrate the mutual actions of magnet-poles, it occurred to the writer that the mutual attractions and repulsion of currents might be illustrated in a similar manner by the figures formed with iron filings. He was aware† at that time that the lines of force of a straight conductor carrying a current were a series of concentric circles lying in a plane to which the conductor was normal. The series of figures now published originates, therefore, with the discovery of Faraday that the seat of the mutual actions of currents and of magnets must be sought in the surrounding medium. Since the communication of the preliminary notice, the writer has learned that one or two of the figures had been previously and independently observed by Professor F. Guthrie, but not published. Two others, Nos. 4 and 5 of the present series, are imperfectly given by Faraday in figures 18 and 19 of plate iii. in the third volume of his 'Experimental Researches' (Series Twenty-ninth)‡, and without reference to the conclusions to be derived from their forms, which Faraday apparently overlooked§.

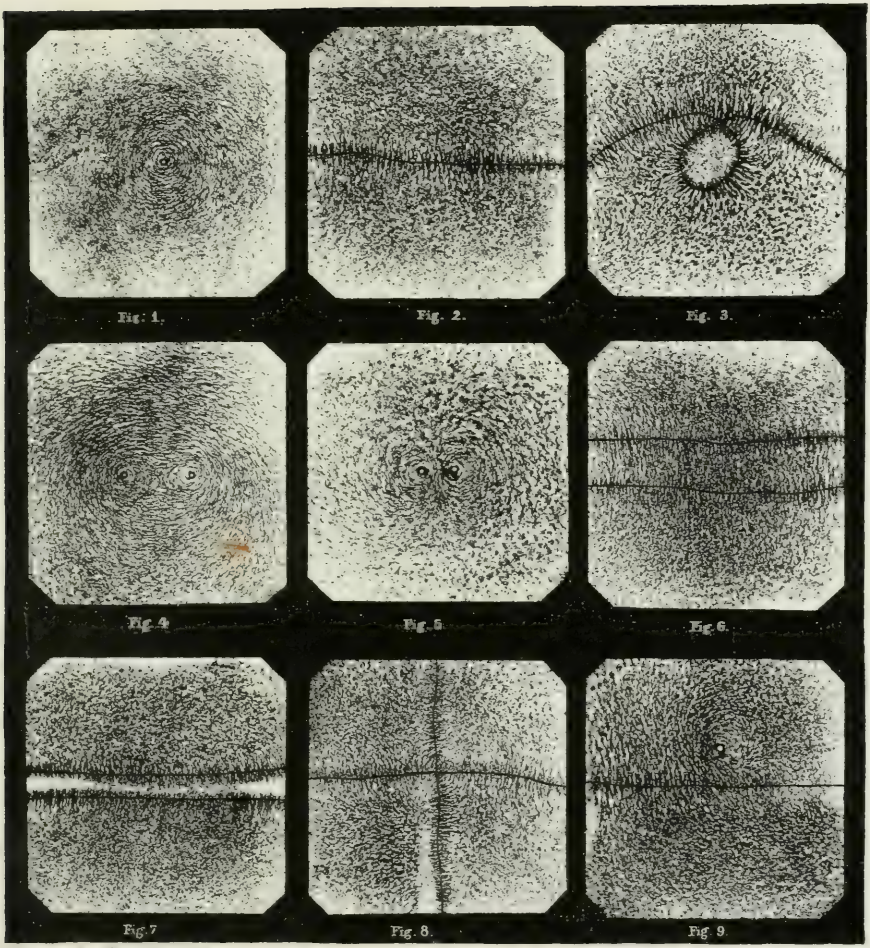
The method employed for preserving the figures has been uniform throughout the series. Plates of glass, $3\frac{1}{2}$ inches long by $3\frac{1}{4}$ inches broad, were coated with a solution of gum-arabic and gelatine, and were then carefully dried. When the ar-

* Communicated by the Physical Society.

† See Faraday, 'Experimental Researches in Electricity,' vol. iii. p. 400, § 3239, and plate iii. fig. 17; Guthrie, 'Magnetism and Electricity,' p. 254, fig. 225; Clerk-Maxwell, 'Electricity and Magnetism,' vol. ii. art. 477.

‡ And 'Phil. Trans. 1852, p. 137.

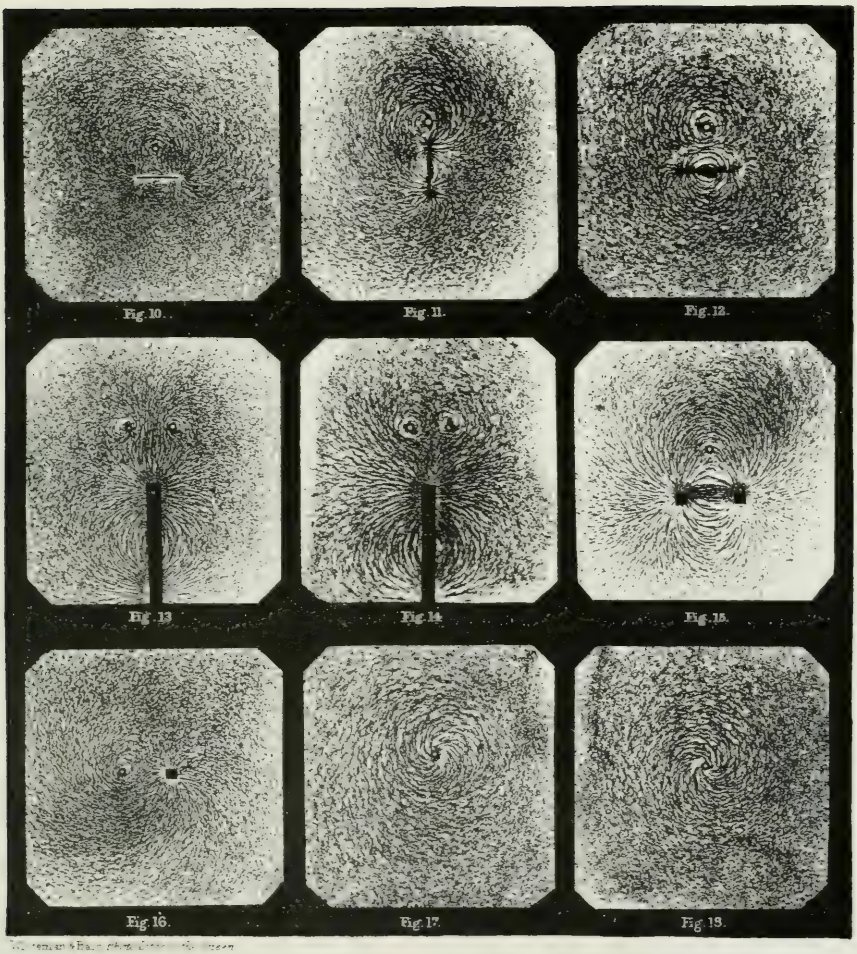
§ The attention of the writer has also been drawn to a statement in the American Journal of Science for 1872, p. 263, by Professor A. M. Mayer, that he has obtained magnetic "spectra" from electric currents in a manner somewhat similar to that now described. The figures have, however, remained unpublished and undescribed, so that the writer has no means of learning how far the substance of the present communication may have been anticipated.



Wherrett & Bass, Photo-Litho to the Queen.

MAGNETIC FIGURES

ILLUSTRATING ELECTRO-DYNAMIC RELATIONS.



MAGNETIC FIGURES

ILLUSTRATING ELECTRO-DYNAMIC RELATIONS.

rangement of magnets or of conducting-wires had been made for the particular case of the experiment, and the plate been laid in a horizontal position, fine filings of wrought iron previously sifted were dusted over the plate through muslin, and the plate was tapped lightly with vertical blows from a piece of thin glass rod. When the filings had arranged themselves, and the plate was still *in situ*, a gentle current of steam was allowed to play upon the plate, condensing upon the surface of the gum and softening it, and thus allowing the filings to embed themselves where they lay. After the gum had again become hard, the prepared face was covered by a protecting plate of glass, on which in certain cases were drawn the positions of the wires or magnets employed. The figures fixed in this manner are suitable for projection with the lantern upon the screen. They can be readily photographed for transparencies, or for paper prints; specimens of each of these methods of photographic reproduction are exhibited to the Society.

Figure 1 represents the condition of the magnetic field surrounding the current in a straight conducting-wire, which was carried vertically through a hole drilled in the plate. The wire employed throughout the series was a silver one of about ·8 millim. in diameter. The battery power employed for this experiment was that of 20 Grove's cells arranged in two series of ten each. In some of the succeeding experiments a less current was found sufficient.

But for the imperfections of the method of experiment, these curves would be perfect circles, and the distances between two successive lines of force would be proportional to the square of the distance from the central point. The equipotential magnetic surfaces, being always normal to the magnetic lines of force, would be represented by a system of radial lines forming equal angles with one another. There appears to be no recognized name for the closed curves traced out by the lines of force around conductors carrying currents. With great diffidence I therefore beg to speak of them as isodynamic lines. They are theoretically disposed about a single straight conductor in a perfectly concentric manner, and at such distances apart as would be defined by the requirement that a parallel conductor, carrying a like current of unit strength, would do unit work in passing from one isodynamic line to the next. The absolute value of an isodynamic line would of course be determined (like magnetic and electrostatic potential) by the work done by a like element of current in passing to any point in that line from an infinite distance. No work is done in moving an element of a parallel current along an isodyna-

mic line, just as no work is done in moving a magnet-pole along in an equipotential surface. The isodynamic lines occupy, therefore, exactly the same relation to the element of the circuit, as do the equipotential surfaces to a magnet-pole or to an electrified point.

Figure 2 represents the field above a horizontal wire carrying a current, and separated from the filings by the thickness of the glass (about 1·7 millim.). The lines cross the wire at right angles, and are really the projections of a series of such circles as exist in figure 1.

Figure 3 exhibits the form assumed by the filings when the wire beneath the plate was coiled into a simple loop, a small piece of mica being inserted to prevent contact where it recrossed its path. The lines of the field within the loop run longitudinally ; and their projections on the surface are mere points, as the filings show.

In figures 4 and 5, two wires pass vertically through the plane of the figures, carrying parallel currrents, which in figure 4 are in the same direction, in figure 5 in opposite directions. Ampère's well-known law of the attraction in the former case, and of the repulsion in the latter, is well illustrated by the forms of the magnetic curves. In the former, where the parallel currents attract, the outer isodynamic lines are closed curves embracing both centres, the inner are distorted ovals about each centre—the whole forming a system of lemniscates, as would necessarily be the case, since the attraction at any point in the plate varies inversely as the square of the distance from each current*.

In figure 5, where the parallel currents repel each other, the lines of force due to either current in no case enter or coalesce with those of the other current. They form two series of ovals of a peculiar form, flattened on the sides presented towards the opposing series.

The conception of Faraday, "that the lines of magnetic force tend to shorten themselves, and that they repel each other when placed side by side," has been shown by Clerk-Maxwell, who thus concisely states it, to be perfectly consistent with the theory that explains electromagnetic force as the result of a state of stress in the medium filling the surrounding space†. Faraday also observes that "unlike magnetic lines, when end on, repel each other, as when similar poles are face to face," and that "like magnetic lines of force," when end on to each other, coalesce. The terms "like" and "unlike," as applied

* See Thomson and Tait, 'Natural Philosophy,' art. 508, vol. i. p. 382.

† Clerk-Maxwell, 'Electricity and Magnetism,' vol. ii. art. 645; Faraday, 'Experimental Researches,' 3266, 3267, 3268.

to magnetic lines of force, refer, of course, to the two cases of similarly or oppositely directed lines, the positive direction of a line of force being reckoned as the direction in which a north-seeking pole on it would tend to move *. The mutual coalescence or repulsion exerted between the lines of force of unlike or like magnetic poles is familiarly employed in the experimental illustration of magnetic phenomena, so well known since the researches of Professor Robison and Dr. Roget on the magnetic curves, and appears to have been recognized long before†. It is believed that the present is the first distinct attempt to apply similar considerations to the illustration of electrodynamic relations between systems of conductors carrying currents, and between conductors of currents and magnet-poles.

The isodynamic lines, which are lines of magnetic force, tend to shorten themselves. A very hasty inspection of fig. 4 will show that if any one of the system of lemniscates were to "shorten itself," it would tend to bring the two centres nearer together. Consider each isodynamic line as a ring of some elastic material (as, for example, an indiarubber ring) stretched around a bundle of smooth wires, the cross section of the bundle having a perimeter corresponding in form to the isodynamic line under consideration. The maximum shortening of such an elastic ring would take place when the enclosed area was made a circle. In other words, the lemniscate-form isodynamics tend to become circles, and the two like parallel currents are mutually urged towards each other.

In figure 5 the shortening of the isodynamic lines, and their approach to the truly circular form, could only be accomplished by the wider separation of the two conductors from each other. Hence the mutual repulsion of two parallel conductors carrying oppositely directed currents.

Figures 6 and 7 show the lines of force above the parallel currents when these pass horizontally below the glass. In the case of like currents the lines coalesce. In the case of unlike currents they repel each other, and pass between the two wires in a direction vertical to the plane of the glass, where their characteristic form, as lines, is lost. The observation that the filings adherent to wires carrying like parallel currents are mutually attractive appears to have been first made by Davy.

Figure 8 illustrates the law of currents crossing one another at a point. In the two quadrants in which the currents both

* See Clerk-Maxwell, 'Electricity and Magnetism,' art. 489; L. Cumming, 'Theory of Electricity,' p. 194.

† See Musschenbroek, *Dissertatio Physica Experimentalis de Magnete*, cap. iv. exp. cxvii., and tab. iv. figs. 4 & 5.

run to or from the central point, the lines of force tend to coalesce. In the alternate quadrants they mutually repel each other; and the angle of these quadrants tends to increase.

In figure 9 one current is carried horizontally below the glass, the other traverses the plane of the figure normally. The dissymmetry of the resultant distribution of the lines reveals the dissymmetrical nature of the force which tends to bring the currents into parallelism. Any shortening of the isodynamic lines would tend to move the vertical current from that quadrant over which the lines are unbroken. Figures have also been obtained with two currents crossing the plate in a vertical plane of incidence, but each at 45° to the normal. With some distortion, these figures bear a general resemblance to figs. 4 and 5 in the two cases of the currents passing through the plate in similar or in opposed directions. In the former case their angular separation tends to diminish, in the latter to increase.

Figures 10, 11, and 12 introduce the action of a vertical current upon a small magnetic needle lying on the glass plate. In the first case the needle lies in stable equilibrium almost tangentially to the isodynamic lines; in the third its position is reversed and unstable; in the second case it is set at right angles to the directive action of the current. The dissymmetrical action of the forces on its poles produces a couple tending to turn it about its centre, as would be inferred from an inspection of the lines of force of the figure.

Figures 13 and 14 illustrate a deduction from the theory of the magnetic shell. A conductor carrying a current is acted upon by a force urging it forward so as to make the number of like lines of force included within it a maximum*; that is to say, a north-seeking pole is attracted into a circuit on the side from which the positive current appears to circulate in a right-handed cyclical order (or in the same direction as the hands of a clock). Similarly the circuit is urged backwards from a contrary pole, and tends to make the number of unlike lines of force included within it a minimum. In the figures a current ascends through the plane of the figure on the left, and descends through it on the right, in a right-handed cyclical order as seen from the magnet. Hence a north-seeking pole is attracted, and a south-seeking pole repelled.

Figure 15 results from the action of two magnet-poles upon a vertical conductor, which in this case is attracted between the poles.

Figure 16 illustrates the mutual tendency to rotation between a magnet-pole and a conductor carrying a current parallel to

* Clerk-Maxwell, 'Electricity and Magnetism,' art. 490.

the axis of the magnet. In the figure, where the vertical current passes upwards through the glass, the neighbouring south-seeking pole (marked in position by a square dot) is urged round the current with a couple tending in a right-handed cyclical rotation. The couple is reversed, and acts in a left-handed order, if either the current or the magnet-pole be reversed. The contrasted dissymmetry so produced is very curious, and the mutual displacement of the radial lines of force of the pole and of the circular lines of the current is very significant.

Figures 17 and 18 show lines of force arranged spirally in the field. In these a current passes upwards through the glass, while the pole of a magnet is placed vertically beneath: the current, in fact, passes through the magnet. The form of the lines of force is remarkable. No work would be done on or by an element of a vertical current in bringing it up to the centre along one of the spiral lines; for the work done *by* it in bringing it in the spiral path across the successive circular isodynamic lines of the current would be equal to that done *upon* it in carrying it across the successive radial lines of force of the magnet-pole. The equipotential surfaces of this field are consequently another series of spirals of an opposite cyclical order. In figure 17 the current running up through a south-seeking pole produced a right-handed spiral; in figure 18, with a north-seeking pole the spiral is of the opposite order. Since the magnetic potential decreases from a magnet-pole with the inverse square of the distance, and since the inductive action of the current on a point in the plane of the figure also decreases according to the inverse square of the distance from the current, each branch of the spirals would be described by a moving point whose angular displacement from the arbitrary zero is simply proportional to the distance from the central point. The results of actual measurement of the spirals at successive distances of whole millimetres from the centre show as near an agreement with this supposition as the roughness of the method of procuring the curves permits. The number of branches of the spiral will clearly be proportional to the strength of the magnet pole. The obvious result of a "shortening" of the spiral lines would be to produce a rotational movement, such as we know to be produced on a free-magnet pole under the influence of a current traversing it longitudinally.

I am indebted to Mr. Robert Gillo, of Bridgwater, for the admirable photographic copies of the various figures.

June 19, 1878.

XLVII. *On the Applicability of* Lagrange's *Equations in certain Cases of Fluid-Motion.* *By* JOHN PURSER, *M.A., Professor of Mathematics in the Queen's College, Belfast**.

THE ordinary condition for the applicability of Lagrange's equations to the motion of a system is that the position of all its parts be determined as a function of the generalized coordinates or parameters which enter into these equations. When such is not the case, even though the kinetic energy may be expressible in terms of these coordinates and their differential coefficients with respect to the time, the Lagrangian equations of motion are known not to be in general valid.

A familiar illustration of this is afforded by the motion of a rigid body rolling on a plane so rough as to prevent sliding. Here it is evident that the kinetic energy can be expressed in terms of three coordinates defining the angular position of the body and their differential coefficients. It is not, however, possible to express the position of all the points of the body in terms of these coordinates only; and accordingly the use of Lagrange's equations in terms of these coordinates is known to lead to erroneous results. It becomes, therefore, a matter of considerable interest to inquire into the grounds which justify recent important applications of these equations to sundry problems of hydrokinetics, relating to the motion of rigid bodies in an incompressible frictionless fluid.

This case, in fact, is in so far similar to that of the rolling body already alluded to, that while the kinetic energy of the system can be expressed (in virtue of Green's theorem) in terms of the limited number of parameters which define the position of the rigid bodies, it is clear from the smallest consideration that these parameters do not determine the position of the particles of the fluid. It would certainly seem, then, that we are not entitled *primâ facie* to assume the validity of Lagrange's equations when applied to such problems, and that, if their use be here justifiable, it must be in virtue of special reasons. To endeavour to supply the proof which is thus seen to be requisite is the object of the present communication.

Given a number of rigid bodies moving in a frictionless incompressible fluid, whether infinitely extended or enclosed in a rigid envelope; given also that the motion at one epoch is irrotational, and therefore always so,—then by D'Alembert's principle,

$$\Sigma dm\{\ddot{x}\delta x+\ddot{y}\delta y+\ddot{z}\delta z\}+\delta V=0, \quad . \quad . \quad (A)$$

* Communicated by the Author, having been read before the British Association at Dublin, August 1878.

where V denotes the potential energy due to the applied forces which act on the bodies. δx, δy, δz, as far as they apply to the particles of the bodies, can, of course, be expressed in terms of δq_1, δq_2, &c., the variations of the generalized coordinates determining the position of the bodies. And the same holds true for the particles of the fluid, provided we suppose the displacements irrotational. The above equation may therefore be written

$$Q_1\delta q_1 + Q_2\delta q_2 \ldots + \frac{dV}{dq_1}\delta q_1 + \frac{dV}{dq_2}\delta q_2 \ldots = 0\,;$$

and the equations of motion are

$$Q_1 + \frac{dV}{dq_1} = 0, \quad Q_2 + \frac{dV}{dq_2} = 0, \text{ \&c.}$$

To obtain Q_1, suppose

$$\delta q_2 = 0, \quad \delta q_3 = 0, \text{ \&c.},$$

then

$$\Sigma dm(\ddot{x}\delta x + \ddot{y}\delta y + \ddot{z}\delta z)$$

$$= \Sigma dm \frac{d}{dt}(\dot{x}\delta x + \dot{y}\delta y + \dot{z}\delta x)$$

$$- \Sigma dm\left(\dot{x}\frac{d}{dt}\delta x + \dot{y}\frac{d}{dt}\delta y + \dot{z}\frac{d}{dt}\delta z\right).$$

Now if

$$\delta x = a_1\delta q_1, \quad \delta y = b_1\delta q_1, \quad \delta z = c_1\delta q_1, \text{ \&c.},$$

when a_1, b_1, c_1 are functions of the q's and the coordinates x, y, z, then

$$\dot{x} = a_1\dot{q}_1, \quad \dot{y} = b_1\dot{q}_1, \quad \dot{z} = c_1\dot{q}_1, \text{ \&c.};$$

$$\therefore\ a_1 = \frac{d\dot{x}}{d\dot{q}_1}, \quad b_1 = \frac{d\dot{y}}{d\dot{q}_1}, \text{ \&c.};$$

$$\therefore\ \Sigma dm \frac{d}{dt}(\dot{x}\delta x + \dot{y}\delta y + \dot{z}\delta z)$$

$$= \delta q_1 \Sigma dm \frac{d}{dt}\left(\dot{x}\frac{d\dot{x}}{d\dot{q}_1} + \dot{y}\frac{d\dot{y}}{d\dot{q}_1} + \dot{z}\frac{d\dot{z}}{d\dot{q}_1}\right) = \delta q_1 \frac{d}{dt}\left(\frac{dT}{d\dot{q}_1}\right).$$

If now we are justified in assuming that

$$\Sigma dm\left(\dot{x}\frac{d}{dt}\delta x + \dot{y}\frac{d}{dt}\delta y + \dot{z}\frac{d}{dt}\delta z\right)$$

$$= \Sigma dm(\dot{x}\delta\dot{x} + \dot{y}\delta y + \dot{z}\delta^z),$$

then, since this latter expression $= \delta q_1 \frac{d\mathrm{T}}{dq_1}$, we get at once Lagrange's equation,

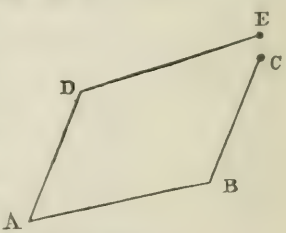
$$\frac{d}{dt}\left(\frac{d\mathrm{T}}{d\dot{q}_1}\right) - \frac{d\mathrm{T}}{dq_1} + \frac{d\mathrm{V}}{dq_1} = 0.$$

It remains only to consider why the above assumption is legitimate. As far as the integral applies to the particles of the rigid bodies, since $\frac{d}{dt}\delta x = \delta \frac{dx}{dt}$ for each particle, the transformation is obviously justified. For the fluid, on the other hand, it is evident that, for an individual particle, $\frac{d}{dt}\delta x$ is *not* $= \delta \frac{dx}{dt}$. (This may be seen at once by considering the case of the motion of a flat piece of cardboard in a fluid, and supposing the time displacement perpendicular to its plane and the arbitrary displacement in its plane.)

To examine the meaning of the differences $\left(\frac{d}{dt}\delta x - \delta \frac{dx}{dt}\right) dt$, &c., let us suppose that the generalized coordinates are so taken that one only of the coordinates, say q_2, alters with the time, so that the actual time displacement may be treated in the same way as a possible displacement δq_2.

The above may accordingly be written $\delta_2\delta_1 x - \delta_1\delta_2 x$, where δ_1, δ_2 correspond to the variations δq_1, δq_2.

Consider any point of the fluid A. Suppose the dispacement δq_1 followed by the displacement δq_2, and let the answering positions of A be B and C. Again, suppose the displacement δq_2 followed by δq_1, and let the answering positions of A be D and E. E will not coincide with C, but the displacement is that which has for its projections the expressions above.



For, projection of EC = projection of DE − projection of AB
− (projection of BC − projection of AD)

$$= \delta_2\delta_1 x - \delta_1\delta_2 x.$$

The *bodies*, however, after these two compound displacements are in identical positions ; and consequently the displacements of the fluid $(\delta_2\delta_1 - \delta_1\delta_2)x$, &c., correspond to irrotational displacements of the fluid compatible with a position of the

bodies momentarily fixed. We have then to show that, for such a displacement,

$$\Sigma dm(\dot{x}\delta x+\dot{y}\delta y+\dot{z}\delta z)=0.$$

This is evident; for the work done by the momenta of the particles of the system must for any possible displacement be equal to that done by the impulse; and the latter in this case vanishes, as the bodies remain fixed.

The same thing may be shown analytically thus:—

$$\begin{aligned}&\Sigma dm(\dot{x}\delta x+\dot{y}\delta y+\dot{z}dz)\\&=\Sigma dm\left(\frac{d\phi}{dx}\delta x+\frac{d\phi}{dy}\delta y+\frac{d\phi}{dz}\delta z\right)\\&=\iint d\mathrm{S}\,.\,\rho\,.\,\phi\,.\,\delta n\\&-\iiint dx\,dy\,dz\,.\,\rho\,.\,\phi\,.\left(\frac{d\delta x}{dx}+\frac{d\delta y}{dy}+\frac{d\delta z}{dz}\right).\end{aligned}$$

The first term vanishes, since the motion of the fluid in contact with the bodies is tangential; the second, since the fluid is incompressible.

Addendum.

Sir William Thomson has shown, in his paper on Vortex-Motion (Trans. R. S. E. vol. xxv.), that if one or more solid bodies are moving in an infinitely extended frictionless incompressible fluid, the motion of the fluid being supposed at one instant and therefore always irrotational, the *impulse* (*i. e.* the system of forces which would at any instant, if applied to the solids, generate the motion of the system of solids and fluids) would, if applied to a rigid body, represent a constant motive. It may be interesting to show that this conclusion follows directly from the Lagrangian equations.

First, let there be only one rigid body. Take two systems of coordinate axes—the first (OX, OY, OZ) fixed in space, the second (O′X′, O′Y′, O′Z′) attached to the body.

Let u, v, w be the components of the velocity of O′ estimated along the moving axes;
p, q, r the rotations of the body round these axes;
ϕ, ψ, θ the usual angles denoting the position of the moving axes with respect to the fixed axes;
x, y, z the coordinates of O′ with respect to the fixed axes.

Then, taking as generalized coordinates x, y, z, ϕ, ψ, θ, we

should have, were continuous forces applied to the body,

$$\frac{d}{dt}\cdot\frac{d\mathrm{T}}{d\dot{x}}-\frac{d\mathrm{T}}{dx}=\text{ } x\text{-component of forces,}$$

$$\frac{d}{dt}\cdot\frac{d\mathrm{T}}{d\dot{\psi}}-\frac{d\mathrm{T}}{d\psi}=\text{ moment of forces round O'Z}_{,},$$

O'Z$_{,}$ being a parallel through O' to OZ.

It follows, integrating through the short interval of time during which the instantaneous impulse would act, that

$$\frac{d\mathrm{T}}{d\dot{x}}=\text{ component of impulse along OX,}$$

$$\frac{d\mathrm{T}}{d\dot{y}}=\text{ }\quad,,\quad\text{ }\quad,,\quad\text{ OY,}$$

$$\frac{d\mathrm{T}}{d\dot{z}}=\text{ }\quad,,\quad\text{ }\quad,,\quad\text{ OZ,}$$

$$\frac{d\mathrm{T}}{d\dot{\psi}}=\text{ component of moment of impulse round O'Z}_{,}.$$

In the actual motion

$$\frac{d}{dt}\cdot\frac{d\mathrm{T}}{d\dot{x}}-\frac{d\mathrm{T}}{dx}=0;$$

$\therefore$, since T does not depend on x, y, z,

$$x\text{-component of impulse}=\frac{d\mathrm{T}}{d\dot{x}}=\text{ constant,}$$

and similarly for the y-component and the z-component.

Let us now proceed to find the physical meaning of $\frac{d\mathrm{T}}{d\dot{\psi}}$, and interpret the equation

$$\frac{d}{dt}\cdot\frac{d\mathrm{T}}{d\dot{\psi}}-\frac{d\mathrm{T}}{d\psi}=0.$$

It is clear from Green's theorem that T can be expressed as a quadratic function of p, q, r, u, v, w with *constant* coefficients;

$$\therefore\ \frac{d\mathrm{T}}{d\dot{\psi}}=\frac{d\mathrm{T}}{dp}\cdot\frac{dp}{d\dot{\psi}}+\frac{d\mathrm{T}}{dq}\cdot\frac{dq}{d\dot{\psi}}+\frac{d\mathrm{T}}{dr}\cdot\frac{dr}{d\dot{\psi}}$$
$$+\frac{d\mathrm{T}}{du}\cdot\frac{du}{d\dot{\psi}}+\frac{d\mathrm{T}}{dv}\cdot\frac{dv}{d\dot{\psi}}+\frac{d\mathrm{T}}{dw}\cdot\frac{dw}{d\dot{\psi}}.$$

When p, q, r are expressed in terms of ϕ, ψ, θ, their coefficients do not involve the precessional angle ψ. Therefore the first three terms vanish.

Again,

$$\frac{d\mathrm{T}}{du}, \quad \frac{d\mathrm{T}}{dv}, \quad \frac{d\mathrm{T}}{dw}$$

are the force-components of the impulse with respect to O′X′, O′Y′, O′Z′;

$$\frac{du}{d\psi}, \quad \frac{dv}{d\psi}, \quad \frac{dw}{d\psi}$$

correspond to the displacement of a vector u, v, w fixed in space with respect to the moving axes, owing to the motion $d\psi$ of the latter;

$$\therefore \frac{d\mathrm{T}}{d\psi} = \mathrm{R} \cdot \frac{\text{component along R of relative displacement of } \begin{bmatrix}\text{vector } u, v, w\end{bmatrix}}{d\psi}$$

$$= -\text{ moment of a force R applied at end of vector } u, v, w \text{ round axis O′Z}_1;$$

∴ Lagrange's equation

$$\frac{d}{dt} \cdot \frac{d\mathrm{T}}{d\dot{\psi}} - \frac{d\mathrm{T}}{d\psi} = 0$$

means that

$$\frac{d}{dt} \text{ (moment of impulse round O′Z}_1\text{)}$$

$$+ \text{ moment of force of impulse applied at end of vector } u, v, w \text{ round the same line} = 0.$$

The left-hand member is obviously

$$= \frac{d}{dt} \text{ (moment of impulse round OZ)};$$

∴ this last moment is constant, and the moments round OX and OY are also constant.

Secondly, let there be more rigid bodies than one. We can now assume as the generalized coordinates the same as we have just taken which have reference to one of the bodies, together with other coordinates depending entirely upon the relative position of the rigid bodies amongst themselves; then $\frac{d\mathrm{T}}{d\dot{x}}$ and $\frac{d\mathrm{T}}{d\dot{\psi}}$ are evidently the force-components along the axis OX and the moment round the axis O′Z$_1$ of the whole impulse, and the reasoning runs as before.

XLVIII. *Researches on Unipolar Induction, Atmospheric Electricity, and the Aurora Borealis.* *By* E. Edlund, *Professor of Physics at the Swedish Royal Academy of Sciences.*

[Continued from p. 306.]

§ 2. *Atmospheric Electricity and the Aurora Borealis.*

It is known that the earth may be regarded as a relatively good conductor of electricity; while, on the other hand, the atmospheric air, in the dry state and under the pressure to which it is subject at the surface of the earth, is a very bad conductor. Its conductivity, which depends almost exclusively upon the relative quantity of humidity which it contains, is consequently subject to incessant variations from the double point of view of time and space. When the density of the air diminishes, its conductivity increases; consequently there must exist at a considerable altitude above the terrestrial surface a stratum of air the conducting-power of which is better, yet without being particularly good. The terrestrial surface, both solid and liquid, is therefore immediately surrounded by a stratum of air endowed with feeble conductivity and subject to incessant variations. To this stratum succeeds another, the conductivity of which is greater and, as far as we know, sensibly invariable. The upper limit of the atmosphere has been fixed by astronomic methods at an altitude of between 70 and 80 kilometres. Truly speaking, these determinations signify only that the atmosphere up to that limit possesses sufficient density for its presence to be indicated by the ordinary methods of determination. That the atmosphere, though excessively rarefied, extends to a still greater elevation is most evidently proved by the fact that shooting stars have been observed at nearly 900 kilometres above the surface of the earth. These small bodies evidently can only become bright in consequence of a portion of their *vis viva*, transformed into heat by the friction of the air, augmenting their temperature to such a degree that they begin to shine. Now we can only perceive the falling body from the moment when it becomes luminous; and it is clear that at that moment it will have already traversed a certain length of path in the rarefied atmosphere before attaining so high a temperature. Therefore the upper boundary of the atmosphere must be situated at a much greater distance from the earth than has hitherto been admitted.

The magnetic action of the earth cannot be explained entirely and in detail by the assumption that its magnetic force is due to a magnet of iron or steel situated in the earth, and

making a certain angle with the axis of rotation. The said action is too irregular for such an explanation to be admissible. Nevertheless the total intensity of the magnetic force is ascertained to increase in a sufficiently regular manner as we recede from the magnetic equator towards the magnetic poles. If we join the points of the terrestrial surface where the magnetic dip has the same value, we obtain curves which, though not forming true circles encompassing the earth and parallel to one another, may yet be regarded as parallel circles drawn at the surface of the earth, and having their centres on the right line which joins the earth's magnetic poles. If from the equator we proceed continually in the direction indicated by the declination-needle (not in such a direction that the angle of declination remains constantly the same), we obtain magnetic meridians converging towards the magnetic poles; these meridians are not great circles, but yet bear a certain resemblance to them*. In general, and on a large scale, it is therefore permissible to regard the earth as a magnet the axis of which makes a certain angle with the terrestrial axis of rotation. There is no need of a more exact idea of the magnetic condition of the earth for the exposition which follows.

Let *abcd* (fig. 9) be a section passing through the axis of rotation of the earth (which we will suppose to constitute a perfect sphere); *ac* is its rotation-axis, *bd* its equator, and *o* its centre. To simplify the question, we will at first suppose that the two magnetic poles are on the rotation-axis, the south pole in *s*, and the north pole in *n*. *abod* will consequently represent the northern hemisphere; and we will designate by $a'b'c'd'$ the upper limit of the atmosphere. While the earth is turning from west to east about its axis, an electric molecule situated in *m* describes in the same direction a circle parallel to the equator, and therefore forms a current which is acted upon by the two poles of the magnet. If now we pass planes through the circuit-element situated in *m* and through the two poles of the magnet, these planes will cut the figure along the right lines *sm* and *nm*; and if in the same planes we draw the right lines *mp* and *mq* respectively perpendicular to *ms* and *mn*, we shall get the directions in which the magnetic poles tend to direct the positive electric molecule (the æther).

Let r denote the distance from the centre of the earth to *m*, ρ the distance from each of the poles of the magnet to the same centre, and l the latitude of the point *m*. We remark at starting that the magnetic phenomena presented by the earth indicate that at least ρ cannot exceed in length the half of the earth's radius; and, in consequence, we assume that ρ

* Becquerel, *Traité complet du Magnétisme* (Paris, 1846), p. 428.

is at most equal to the half of that radius. The squares of the distances of the two poles from the point m will therefore be respectively

$$r^2+\rho^2-2r\rho\sin l \text{ and } r^2+\rho^2+2r\rho\sin l.$$

The intensity of the current is proportional to the velocity with which the molecule m moves in its parallel circle; and that velocity is, in its turn, proportional to the distance from the rotation-axis, consequently to $r\cos l$. Designating the intensity of the magnetic poles by M, and by k a constant, we shall have, for the force with which the south pole tends to direct the molecule along mp, the expression

$$\frac{k\mathrm{M}r\cos l}{r^2+\rho^2-2r\rho\sin l},$$

and, for the action of the north pole upon the same molecule along the line mq,

$$\frac{k\mathrm{M}r\cos l}{r^2+\rho^2+2r\rho\sin l}.$$

Taking the sum of the components of these forces along the earth's radius drawn through the point m, we get

$$\frac{k\mathrm{M}r\rho\cos^2 l}{(r^2+\rho^2-2r\rho\sin l)^{\frac{3}{2}}}+\frac{k\mathrm{M}r\rho\cos^2 l}{(r^2+\rho^2+2r\rho\sin l)^{\frac{3}{2}}}. \quad . \quad . \quad . \quad (\mathrm{A})$$

This sum, which we will name the vertical component, denotes the force with which the magnet tends to direct the æther (the electropositive fluid) upward in a vertical direction. (If we assume also an electronegative fluid, this will be urged by the same force in the opposite direction.)

If now we consider an electric molecule which is situated in the atmosphere or at the surface of the earth, for which, therefore, r is $\geq 2\rho$, we see that formula (A) will be equal to zero at the polar point, and will possess a relatively minimal value in the vicinity of that point. Consequently the force tending to carry the electric molecule vertically upwards is *nil* at the pole, and a minimum in the polar region. It follows of course, and is besides proved by the formula, that the sum is equal for the same latitudes in both hemispheres.

Taking the component of these forces in a direction making a right angle with the earth's radius (the tangential component), we obtain the force with which the electric molecules are urged along the tangent of the circle the radius of which is r. We have thus:—

$$\frac{k\mathrm{M}r(r-\rho\sin l)\cos l}{(r^2+\rho^2-2r\rho\sin l)^{\frac{3}{2}}}-\frac{k\mathrm{M}r(r+\rho\sin l)\cos l}{(r^2+\rho^2+2r\rho\sin l)^{\frac{3}{2}}}. \quad . \quad (\mathrm{B})$$

In the equatorial plane this force becomes $=0$. The electric molecules situated in that plane move therefore vertically upward, seeing that the component of the force A is the only one which acts upon them. At the terrestrial poles ($l=90$) B and A are alike equal to *nil*; therefore the molecules situated in the parts themselves undergo no action from the magnet. For all the other molecules, the distance r of which from the centre of the earth is above or equal to 2ρ, the first term of the expression (B) will be always positive. For the molecules of the northern hemisphere (that is to say, for the positive values of the latitude) the first term will be numerically higher than the second, and consequently the total expression will be positive; for the southern hemisphere (that is, for the negative values of l) the first term will be numerically lower than the second, and consequently the entire expression will be negative. For the same value of r, and at an equal latitude, the expression (B) will have the same numerical value in both hemispheres; but this value will be positive in the northern, negative in the southern hemisphere. It appears, then, from the expression (B) of the tangential component of the force, that the electric molecules situated in the terrestrial atmosphere or at the surface of the earth endeavour, in the northern hemisphere, to approach the north pole, and those situated in the southern hemisphere the south pole. The vertical component (A) tends in the same way in both hemispheres to move the molecules always further from the centre of the earth in their course toward the terrestrial poles.

We will now see what is the influence which these forces are capable of exerting upon the electric state of the earth and the atmosphere. The lower stratum of the air is a relatively bad conductor; and we assume at first the ideal case that its conductivity is everywhere equal. The vertical component of the magnetic force in question then tends to direct the æther (positive electricity) from the earth to the air, the lower strata of which consequently become charged with that fluid, while the earth itself, which is a good conductor, incurs a deficit of æther (that is, becomes electronegative). The magnetic force being always in equally intense activity, and the earth rotating with a constant velocity, a portion of the electric fluid is soon conducted into the upper regions of the atmosphere, where the conductivity is better. Arrived there, the electric fluid is impelled towards the poles by the tangential component of the magnetic force. The æther (positive electricity) in this way accumulates in the atmosphere, while the earth itself suffers a deficit of electricity (becomes electronegative). This continues until the electric tension of the atmosphere is suffi-

ciently great to induce a discharge towards the earth. This, as is the case in the ordinary experiments of the laboratory, may take place in two ways—that is, either by an instantaneous discharge, or by a more or less continuous current. This difference of discharge depends on the following circumstances:—

The action of a magnetic pole p upon another magnetic pole q takes effect along the right line which joins p and q; but the action of the magnetic pole p upon a current-element situated at the same point as q, on the contrary, operates in a plane normal to the said right line. The component along this right line of the action of the pole p upon a current-element is consequently equal to *nil*. What has just been said may be applied also to the case of two *magnets* acting upon each other. If, for instance, a magnet sn (fig. 10) acts upon another magnet $s'n'$, and the latter is movable so as to be able to take any position whatever in relation to sn, the magnet $s'n'$ will place itself in the direction of the resultant of the action exerted upon it by the magnet sn. The action of sn upon a *current-element* situated at the point m, or in the same place as $s'n'$, on the contrary, will take place in a plane normal to the same resultant; the component, then, along this resultant of the action of sn upon the current-element is equal to *nil*. If the circle *abcd* represents a section of the earth, the magnet $s'n'$ shows the direction of the dipping needle at the point m, since we suppose $s'n'$ to be in the direction of the resultant of the action exerted upon it by the earth's magnetism. Therefore *the action of terrestrial magnetism upon a current-element situated in the atmosphere has zero for its component in the direction of the dipping needle.*

At the equator the dipping needle takes a horizontal position. The action of the earth's magnetism takes effect here, as the above formulæ show, in a vertical direction upward. If then, the electric fluid of the atmosphere can precipitate itself vertically into the earth, the force which produces this effect must be sufficiently great to overcome not only the electric resistance of the subjacent strata of air, but also the action of the earth's magnetism upon the electric fluid of the atmosphere; or, in other words, the force must exceed the sum of the two obstacles to the motion of the electric fluid. We must further observe that the action of the earth's magnetism in the vertical direction upon a current-element situated in the atmosphere is, according to formula (A), greater at the equator than to the south or north of that circle.

At higher latitudes the dipping needle has not a horizontal position, but makes a greater or less angle with the plane of

the horizon. Here, then, it is possible for the electric fluid of the atmosphere to descend into the earth without the terrestrial magnetism opposing to it an obstacle *directly*, provided it follows the direction of the dipping needle ; but in following that direction the electricity has a longer path to travel to reach the surface of the earth, and consequently suffers a greater resistance than if it could descend vertically. At the magnetic pole the dipping needle takes a vertical position, in consequence of which the resistance opposed by terrestrial magnetism to the propagation of the electricity in the vertical direction is here equal to *nil*. From all this it follows that, *all other circumstances being equal, the resistance to the flow of the electric fluid from the atmosphere to the surface of the earth is greater at the equator and in the equatorial regions than at a certain distance from that circle, and that the resistance diminishes as the dip of the magnetic needle to the earth increases.*

It is here that, in my opinion, we must seek the chief cause of the fact that in the equatorial regions the electric fluid of the atmosphere descends to the earth by strong disruptive discharges, and in high latitudes chiefly by slow and feeble flowings, forming more or less continuous electric currents. If we continue to charge with opposite electricities two insulated bodies at a suitable distance from each other, the electricity at last traverses the intervening space, producing sparks, and the two bodies are discharged. That the discharge thus effected may be powerful and instantaneous, it is necessary that the bodies be good conductors, and the resistance of the intervening space great. If the resistance is slight, the discharge commences while the charge is yet feeble. In proportion as the resistance grows weaker, the discharge takes on more and more the form of a continuous current. In the equatorial regions, or in general in lower latitudes, the force of terrestrial magnetic induction acts with very great intensity in rendering the atmosphere electropositive, and, according to what has just been said, the resistance to a discharge is also very great. When the aqueous vapour of the air condenses so as to form cloud, this becomes charged with the electric fluid accumulated in the air in the locality. The cloud, which is a good conductor, therefore takes an electropositive charge. It is unnecessary to say that negative clouds may also in turn be formed under the inductive influence of positive clouds produced in this way. If now the clouds have become sufficiently electric, the electric fluid may escape to the earth by means of an instantaneous discharge. These discharges, or thunder-claps, then, take place when clouds are formed and when the electric resistance between them and the

earth is as great as is required. Outside of the equatorial regions this resistance is less, as has been said, and violent tempests are more rare. Finally, at a still higher latitude the resistance is so slight that the discharges are transformed into slow and continuous currents, giving rise to the phenomenon called the *aurora borealis*.

That portion of the electric fluid which does not descend into the earth by disruptive discharges in the equatorial regions is carried by the tangential component of the force of induction towards higher latitudes, while its distance from the terrestrial surface is augmented by the vertical component of that force. These currents of electric fluid receive accessions everywhere during their journey towards the poles through the inductive force of the earth's magnetism incessantly impelling into the atmosphere fresh quantities of that fluid from the subjacent terrestrial surface. In proportion as the distance to the poles diminishes, the vertical component of the induction-force also diminishes, and the dipping needle continually approaches nearer to the vertical; and in consequence the resistance opposed by the force of magnetic induction of the earth to the flow of the electropositive fluid to the earth grows less with the diminution of the distance to the poles. When the difference of electric tension between the atmosphere and the earth has become sufficiently great to overcome the resistance opposed by the induction-force of the earth and the subjacent strata of air, the electric fluid flows from the atmosphere to the earth. The places where this phenomenon takes place evidently form a circle around the pole. This circle is characterized by the circumstance that in every point of its circumference the vertical component of the force of terrestrial induction must have nearly the same value. In the vicinity of the poles the atmosphere receives only a feeble charge of electricity, the vertical component of the induction-force being there but very small (as indicated by formula A), and the tangential component directing towards those regions only an insignificant quantity of the electric fluid which enters the atmosphere in lower latitudes.

The electric fluid impelled into the atmosphere by the terrestrial force of magnetic induction descends again therefore in two ways to the earth—either by powerful disruptive discharges, or by more or less continuous feeble currents. The former mode of discharge takes place chiefly in the equatorial regions, the latter especially in high latitudes. The fluid which is not discharged disruptively in the equatorial regions is conducted by the induction-force towards higher latitudes, where the discharge takes place by means of continuous cur-

rents. From this it follows that, the less complete the disruptive discharge in the first-mentioned regions, the more numerous and intense will be the currents in the second.

We assumed above that the terrestrial magnet coincided with the rotation-axis of the earth, and that the earth was everywhere homogeneous and endowed with the same electric conductivity. Now the magnetic poles of the earth are not situated on the axis of rotation; or, in other words, the right line connecting the magnetic poles does not coincide with that axis, but makes with it an angle determined by observations to be about 17°. Moreover the conductivity of the air varies with the time and place. Yet these circumstances necessitate only nonessential modifications in what has been said. The circle *abcd* (fig. 11) represents a section passing through the rotation-axis of the earth and through the right line which joins the magnetic poles. This line makes with the axis the angle α (= about 17°). The distance of the magnetic poles from the axis will therefore be $\rho \sin \alpha$—an expression in which, as already said, ρ cannot exceed the half of the earth's radius.

We will now suppose another plane passing through the axis, and forming the angle v with the preceding plane; and we will consider the action of the magnetic poles upon an electric molecule m situated in this plane. During the rotation of the earth the magnetic poles describe circles the radius of which is $\rho \sin \alpha$. The radius of the circle described in the same time by the molecule m will be $r \cos l$, r denoting the distance of the molecule from the centre of the earth, and l its latitude. The relative velocity of the molecule m with respect to the magnetic pole s will be obtained, according to what precedes, by giving the same velocity to m and to s, but in the opposite direction to that of the already existing velocity of the magnetic pole. If the time of rotation of the earth be taken as unit, that velocity will be denoted by $2\pi\rho \sin \alpha$. The magnetic pole will in this way be brought to rest, and the molecule m will move with the velocity

$$2\pi \sqrt{r^2 \cos^2 l + \rho^2 \sin^2 \alpha - 2r\rho \cos l \sin \alpha \cos v}.$$

The relative velocity of the molecule with respect to the other magnetic pole will be

$$2\pi \sqrt{r^2 \cos^2 l + \rho^2 \sin^2 \alpha + 2r\rho \cos l \sin \alpha \cos v}.$$

Now it is obvious that these square roots denote the distance of the molecule m from the right line drawn through each pole parallel to the earth's rotation-axis. Hence it follows that the magnetic pole acts upon an electric molecule in the same way as if the pole were at rest and the molecule in rotation about

the right line drawn through the magnetic pole parallel to the earth's axis. The squares of the distances between the molecule m and the magnetic poles will be respectively

$$r^2+\rho^2-2r\rho\,(\cos l\sin\alpha\cos v+\sin l\cos\alpha)$$

and

$$r^2+\rho^2+2r\rho\,(\cos l\sin\alpha\cos v+\sin l\cos\alpha).$$

The forces with which the magnetic poles s and n act upon the molecule are therefore expressed by

$$\frac{kM\sqrt{r^2\cos^2 l+\rho^2\sin^2\alpha-2r\rho\cos l\sin\alpha\cos v}}{r^2+\rho^2-2r\rho(\cos l\sin\alpha\cos v+\sin l\cos\alpha)}$$

and

$$\frac{kM\sqrt{r^2\cos^2 l+\rho^2\sin^2\alpha+2r\rho\cos l\sin\alpha\cos v}}{r^2+\rho^2+2r\rho\,(\cos l\sin\alpha\cos v+\sin l\cos\alpha)}.$$

The former of these forces acts in the plane which passes through the molecule m and the right line drawn through the magnetic pole s parallel to the axis of the earth, and the latter in the plane passing through m and the right line drawn, parallel to the same axis, through the magnetic pole n. It will suffice for our purpose to seek the expression of the components of these forces in the cases in which v is equal to 90° and to 0°. This calculation will show that the electric molecule is moved further from the centre of the earth and carried from lower to higher latitudes, that it is situated in the plane represented by fig. 11 or in a plane making a right angle with it. As this must evidently take place whatever the plane in which the electric molecule is situated, the result obtained is that the electric molecules are driven vertically upward and at the same time from lower to higher latitudes. For the highest latitudes, where $\cos l$ is a minimum, both forces, as also their horizontal and vertical components, become very small; the electric density of the polar atmosphere cannot, therefore, be great. Thus, although the position of the magnetic poles is eccentric, the upper regions of the atmosphere from which the electric fluid is precipitated upon the earth in continuous currents must describe a closed annular zone about the pole. But, as we shall demonstrate, this zone is not closed around the astronomic pole as its centre.

We suppose the molecule m situated in the plane passing through the terrestrial axis and through the line joining the poles of the magnet. By making $v=0$ in the preceding formulæ we get the following expressions for the two forces:—

$$\frac{kM(r\cos l-\rho\sin\alpha)}{r^2+\rho^2-2r\rho\sin(l+\alpha)}$$

and

$$\frac{k\mathrm{M}(r\cos l+\rho\sin\alpha}{r^2+\rho^2+2r\rho\sin(l+\alpha)}.$$

Both these forces act in the plane in question—the one along mp, and the other along mq (fig. 11). The cosines of the angles formed by them with the terrestrial radius will be

$$\frac{\rho\cos(l+\alpha)}{\sqrt{r^2+\rho^2-2r\rho\sin(l+\alpha)}}$$

and

$$\frac{\rho\cos(l+\alpha)}{\sqrt{r^2+\rho^2+2r\rho\sin(l+\alpha)}}.$$

Therefore the sum of the vertical forces will be

$$\frac{k\mathrm{M}(r\cos l-\rho\sin\alpha)\rho\cos(l+\alpha)}{[r^2+\rho^2-2r\rho\sin(l+\alpha)]^{\frac{3}{2}}} + \frac{k\mathrm{M}(r\cos l+\rho\sin\alpha)\rho\cos(l+\alpha)}{[r^2+\rho^2+2r\rho\sin(l+\alpha)]^{\frac{3}{2}}}. \quad . \quad (\mathrm{C})$$

On the other hand, the sum of the tangential forces will be

$$\frac{k\mathrm{M}(r\cos l-\rho\sin\alpha)[r-\rho\sin(l+\alpha)]}{[r^2+\rho^2-2r\rho\sin(l+\alpha)]^{\frac{3}{2}}} - \frac{k\mathrm{M}(r\cos l+\rho\sin\alpha)[r+\rho\sin(l+\alpha)]}{[r^2+\rho^2+2r\rho\sin(l+\alpha)]^{\frac{3}{2}}}. \quad . \quad (\mathrm{D})$$

If $l=90^\circ-\alpha$ (or the latitude of the magnetic pole) be introduced into formula (C), the sum of the vertical forces will be $=0$. Therefore the electric molecules in the atmosphere vertically over the magnetic poles are not raised in the vertical direction by the terrestrial magnet.

In the vicinity of the magnetic pole the vertical and tangential components of the induction-force are very small; and consequently *there the electricity of the air must be inconsiderable, just as it was around the astronomic pole when we supposed the coincidence of the two axes.* If into formula (D) the same value of l be introduced, this formula takes a positive value; or, in other terms, the tangential component tends to bring the electric molecules thither from the axis of rotation of the earth. On the contrary, the same formula, when $l=90^\circ$ is introduced into it, acquires a negative value—which signifies that the tangential force-component drives toward the magnetic pole the electric molecules which are in the atmosphere over the astronomic pole. It is therefore between these two points that the point must be situated at which the tangential

force is $=0$. While the vertical component of the earth's induction-force diminishes in general as the latitude increases, the electric tension of the upper strata of the atmosphere, on the contrary, is augmented with the increase of latitude until the tension becomes strong enough to occasion the downflow of the electric fluid into the earth. The annular space of the atmosphere where the electric fluid descends to the earth is evidently closed around the magnetic pole; it is characterized by the circumstance that in it the vertical component of the earth's induction-force has everywhere the same magnitude. Formula (C) gives an idea of its situation.

As we have seen above, the electric fluid flows from the said space to the earth in the direction of the dipping needle (of the inclination or dip of terrestrial magnetism). These electric currents in the rarefied air produce the aurora borealis. It is evident that this phenomenon ought chiefly to be seen in the vicinity of the annular space in question. According to the researches of Mr. Loomis, most of the auroræ boreales appear, in North America, between the latitudes of 50° and 62°, their frequency becoming less at still higher latitudes. The central line of the space in question lies consequently at 56° latitude—that is, at 34° from the astronomic and at 17° from the magnetic pole. This central line is denoted by the point t in fig. 11. We have now to ascertain under what degree of latitude the annular space must be situated at 180° of longitude from there, or, in other terms, between what degrees of latitude auroræ boreales will be most frequent in Europe and Asia.

If in formula (C) we make $l=90°-34°=56°$, we shall have

$$\frac{k\mathrm{M}(r\sin 34-\rho\sin 17)\rho\sin 17}{(r^2+\rho^2-2r\rho\cos 17)^{\frac{3}{2}}} + \frac{k\mathrm{M}(r\sin 34+\rho\sin 17)\rho\sin 17}{(r^2+\rho^2+2r\rho\cos 17)^{\frac{3}{2}}}, \quad . \quad \text{(E)}$$

This expression designates the intensity of the vertical component of the induction-force in North America at 56° latitude.

If in formula (C) we make $l=90°$, we shall have the expression of the same component at the astronomic pole, viz.

$$+\frac{k\mathrm{M}\rho^2\sin^2 17}{(r^2+\rho^2-2r\rho\cos 17)^{\frac{3}{2}}}-\frac{k\mathrm{M}\rho^2\sin^2 17}{(r^2+\rho^2+2r\rho\cos 17)^{\frac{3}{2}}}. \quad . \quad \text{(F)}$$

By r is meant the distance from the centre of the earth to the electric molecule under consideration, situated in the atmosphere; ρ, being (as already observed) less than the earth's semiradius, is consequently also less than $\frac{1}{2}r$. Regard being

had to this circumstance, formula (E) obtains a higher numerical value than (F). From this it follows that the vertical component of the induction-force at the astronomic pole is less than at the point for which formula (E) holds good. If in formula (C) we make $l=90+34$, we obtain the sought force for a point t' situated at 56° latitude counted from d. Formula (C) is in this way transformed into the following :—

$$\frac{kM(r\sin 34^\circ+\rho\sin 17^\circ)\rho\sin 51^\circ}{(r^2+\rho^2-2r\rho\cos 51^\circ)^{\frac{3}{2}}} + \frac{kM(r\sin 34^\circ-\rho\sin 17^\circ)\rho\sin 51^\circ}{(r^2+\rho^2+2r\rho\cos 51^\circ)^{\frac{3}{2}}}. \quad . \quad . \quad (G)$$

Formula (G) having a higher numerical value than (E), the point at which the vertical component of the induction-force will be the same as at the point t must be at a higher latitude than t'. From this it follows that the aforesaid annular zone will cut the plane in question at a point t'' situated between the astronomic pole and t'. Thus the zone presenting the greatest frequency of auroræ boreales must be at a higher latitude in Europe and Asia than in North America.

[To be continued.]

XLIX. *On the Laws of Chemical Change.*—Part I.

By John J. Hood, *Esq.**

WHILE studying chemistry under Prof. Mills, I was much struck by the want of knowledge concerning the laws regulating the amount of change which chemically active bodies undergo in a given time, and in what manner the rate of change is influenced by heat, electricity, &c. Many cases of change have been investigated and represented graphically ; but, as far as I am aware, no theory has been given confirmed by experiment whereby, the temperature and amount of active bodies undergoing change being known, the amount of remaining energy at any time can be calculated. The nearest approach to such a theory was given by Messrs. Harcourt and Esson in the 'Phil. Trans.' for 1867, where they showed that for the case of hydric peroxide reacting on hydric iodide,

$$H_2O_2+2HI=2H_2O+I_2,$$

the amount of change was proportional to the amount of acting substance, considering hydric peroxide as the active body.

* Communicated by the Author.

A thorough investigation on this point might lead to many interesting facts in science, and a clearer insight might be gained into molecular action.

For instance, by a comparison of the rates of change at different temperatures, all other conditions being the same, the necessary data could be obtained to deduce the law of temperature, and so find the point at which no action could take place; or, again, if analogous compounds, such as the sulphates, nitrates, chlorides, &c., have an accelerating or retarding effect on the change, their "equivalence" might be determined or compared—that is to say, whether K_2SO_4, or 174 parts by weight of potassic sulphate, can perform the same amount of work as Na_2SO_4, or 142 parts of sodic sulphate.

The cases of chemical change selected for investigation would require to be under complete control, to allow of the determination of the amount of change up to any period of time, as it might so happen that the intervals of time between two observations would require to be equal in order to calculate the necessary constants required by theory.

The methods of determining the remaining energy should be accurate and speedy.

When two bodies A and B undergo change and produce a third, C, which does not take an active part in the change, it will doubtless, by its mere presence, if not removed from the sphere of action, either accelerate or more probably retard the change taking place; and if this effect be great, supposing C not capable of being removed immediately it is formed, a mathematical statement of the change would not be possible, as the influence of C could not be determined. If, however, the rate of change is so little influenced by the presence of C that it may be neglected, a theory of the action can easily be formed as a guide to the experimentalist.

The experiments detailed in this paper were made merely to see how far the following theory of chemical energy is correct, neglecting all retarding or accelerating effects of the compounds produced during the action. By an inspection of the results, it will be evident that this influence cannot be very large.

Suppose two bodies in solution which are capable of reacting on each other to form new inactive compounds, and the action taking place be expressed by an equation in terms of the time and the amounts of remaining active bodies at that time, on the hypothesis that the amount of change in an indefinitely small space of time is proportional to the product of the remaining active bodies at that time. Let A and B be

the initial values of the bodies, α and β the amounts of A and B that have already undergone change up to time t, and let $\delta\alpha$ be the amount of A acted on in time δt; then, by the above hypothesis,

$$\delta\alpha = \kappa(A-\alpha)(B-\beta)\delta t. \quad . \quad . \quad . \quad . \quad (1)$$

Suppose, further, that the amounts of A and B are chemically equivalent (that is to say, they are just sufficient to render each other inactive), then the ratio

$$\frac{A}{B} = \frac{\alpha}{\beta};$$

call this $\frac{1}{\nu}$, and equation (1) becomes

$$\delta\alpha = \kappa\nu(A-\alpha)^2\delta t. \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

Replacing $A-\alpha$ by y, the amount of A that remains unacted on at the time t,

$$\frac{dy}{dt} = -\kappa\nu y^2, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

which, on integrating, gives

$$\frac{1}{y} = (c+\kappa\nu t),$$

or, writing it in the more convenient form,

$$b = y(a+t), \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (4)$$

being the equation to an equilateral hyperbola with axis t for asymptote.

The influence of temperature and the non-equivalence of A and B I will consider further on, after I show how far experiment agrees with this theory.

Experiments.—In the first experiments made, not knowing how the rate of change was influenced by heat, I took every care to keep the temperature of the water-bath perfectly constant; but in spite of every attention, the fluctuations were about $\pm 1^\circ$ C. This, I afterwards found, could not introduce any considerable error. The flasks containing the experimental solutions were submerged in the bath, and were never removed during the experiment. The solutions were freely exposed to the air, as it was found, after repeated trials, that atmospheric oxygen had not any perceptible influence on them during the time the experiments lasted.

The active bodies used were (1) a solution of ferrous sulphate containing an indefinite amount of hydric sulphate, and (2) a solution of potassic chlorate, the strengths of which were accurately known.

For the determination of the iron, a dilute solution of potassic permanganate was employed, the absolute strength of which was never determined, as the experiments were wholly relative. For the experiments made to find the influence of temperature on the rate of change it was necessary to express the different solutions of permanganate in terms of one standard, this being equivalent to using the same solution for all the experiments.

For measuring the solutions, 10-cubic-centim. and 50-cubic-centim. burettes were employed; the errors of calibration were so small that they were in every case neglected.

Experiment 1.—There were taken 100 cubic centims. ferrous sulphate solution containing ·5772 gram ferrous iron with an indefinite amount of hydric sulphate, 10 cubic centims. potassic chlorate containing ·2104 grm., and 200 cubic centims. water: total volume 310 cubic centims.

·5772 grm. iron is equivalent to ·2105 grm. $KClO_3$, by the equation

$$KClO_3 + 6FeO = KCl + 3Fe_2O_3.$$

All the solutions were immersed in the water-bath until they had acquired the necessary temperature before mixing, the iron solution being first run into the water, then the potassic chlorate, and the whole well shaken.

After standing in the bath five minutes, 10 cubic centims. were withdrawn as rapidly as possible, run into a small flask containing about 20 cubic centims. of water, to partially stop the action going on by the dilution, and the remaining iron determined by means of the permanganate, the whole operation occupying less than one minute. The time was always noted just when the iron solution withdrawn had run out the pipette.

As an excess of permanganate had always to be added to see the tint, ·02 cubic centim. was deducted from the reading of the burette for the coloration; but in many cases no such deduction was made. When the iron to be determined was small, 20 or 30 cubic centims. were withdrawn for titration. The following Table contains the results of this experiment: the numbers under "permanganate calculated" are calculated by theory from the observed times, and those under "time calculated" from the permanganate found—the permanganate found, or y, being the number of cubic centims. required for 10 cubic centims. of the experimental solution.

Taking the first two observations to calculate the constants for

$$y(a+t)=b,$$

we get

$$a=133{\cdot}84, \quad b=1338{\cdot}4.$$

Temperature 16° C.

Permanganate, in cubic centims.		Time, in minutes.	
Found.	Calculated.	Found.	Calculated.
10		0	
8·70		20	
7·67	7·69	40	40·7
6·80	6·79	63	63
5·57	5·58	106	106·4
4·87	4·84	142·4	141
4·43	4·40	170	168·3
3·82	3·81	217	216·5
3·50	3·50	248·5	248·6
3·26	3·24	279	276·7
3·01	3·02	309·5	310·8
2·84	2·81	342	337·4
2·49	2·47	406	403·7
2·06	2·04	520·5	515·9
1·80	1·75	628	609·7
1·77	1·73	639	622·3

Experiment 2.—The solutions employed were 50 cubic centims. ferrous sulphate (equal to ·9847 grm. Fe), 10 cubic centims. potassic chlorate containing ·3593 grm. $KClO_3$, and 400 cubic centims. water : total volume 460 cubic centims. The following Table contains the results of this experiment. Taking the second and third observations for the constants a and b, the equation is

$$y(222{\cdot}8+t)=2512{\cdot}1.$$

Temperature 18° C.

Permanganate, in cubic centims.		Time, in minutes.	
Found.	Calculated.	Found.	Calculated.
11·21	11·27	0	1·3
11·10	.....	3·5	
9·82	.....	33	
8·48	8·52	72	73·3
7·56	7·59	108	109·5
6·76	6·72	151	148·8
6·54	6·54	161	161·3
5·96	5·95	199·3	198·7
5·63	5·65	222	223·4
5·32	5·30	251	249·4
4·98	4·96	283	281·6
4·68	4·68	315	314
4·34	4·33	357	356
4·12	4·11	388	386·9
3·95	3·92	418	413·1
3·76	3·70	455	445·3
3·69	3·67	461	458
3·42	3·40	515	511·7
3·22	3·21	560	557·4
3·03	2·99	617	606·3
2·94	2·92	638	631·6
2·63	2·61	739	732·4
2·34	2·30	869	851
2·13	2·10	972	957

Experiments 3 and 4.—Both these experiments were made at the same time and under similar conditions; each stood five minutes before an observation was made. The solution consisted of 25 cubic centims. ferrous sulphate (equal to ·4923 grm. iron), 5 cubic centims. potassic chlorate, or ·1796 grm., and 200 cubic centims. water.

Taking the second and third observations for the constants for No. 3,

$$y(103{\cdot}1+t)=967{\cdot}23,$$

and No. 4,

$$y(104{\cdot}5+t)=976{\cdot}1.$$

The results are tabulated below.

Temperature 20° C. No. 3.

Permanganate, in cubic centims.		Time, in minutes.	
Found.	Calculated.	Found.	Calculated.
9·34	9·38	0	·4
8·60	.....	9·4	
6·77	.....	39·8	
5·60	5·58	70	69·6
4·70	4·68	103·5	102·7
3·92	3·87	146·5	143·6
3·30	3·26	193·6	190
2·73	2·70	254·5	251·2
2·34	2·31	316	310·2
2·04	2·02	377	371
1·69	1·66	479	469·2
1·53	1·52	532	529
1·28	1·26	661	652·2

No. 4.

Permanganate, in cubic centims.		Time, in minutes.	
Found.	Calculated.	Found.	Calculated.
9·30	9·34	0	·4
8·19	.....	14·7	.....
6·65	.....	42·3	.....
5·46	5·45	74·5	74·2
4·59	4·58	108·5	108·1
3·84	3·83	150	149·6
3·24	3·23	197·5	196·7
2·67	2·69	259	261
2·33	2·31	318	314·4
2·00	1·97	390	383·5
1·67	1·66	483	480
1·50	1·51	542	546·2
1·25	1·26	668,	676·3

As a criterion of the probability of the supposed law, I have calculated the areas enclosed between the curve $y(a+t)=b$, axis y, and asymptote t, with the limits for time of last observation and zero, on the supposition that the law holds good between the two observations selected to determine a and b.

Thus

$$\text{area} = \int_0^{\mathrm{T}} y\,dt$$
$$= b \log_\epsilon \left(1+\frac{\mathrm{T}}{a}\right),$$

or

$$b \log_\epsilon 10 \log_{10}\left(1+\frac{\mathrm{T}}{a}\right),$$

taking $\log_\epsilon 10 = 2{\cdot}3026$.

The areas were also calculated from the experimental numbers by the formula

$$\text{area} = \Sigma \left(\frac{y_{n+1}+y_n}{2}\right)(t_{n+1}-t_n),$$

which is approximately true, independently of any law. The results are given below with the percentage differences of "found" from theory. As the curve is convex to axis t, it is evident that the areas calculated by the latter formula should be slightly greater than theory.

Areas.

	Theory.	Found.	Percentage difference.
No. 1	2346·8	2362·4	·66
„ 2	4219	4239·9	·50
„ 3	1937·3	1962·5	1·24
„ 4	1952·6	1964·7	·62

The question now arises, What will be the form of the equation representing the change when there is an excess of either of the active bodies present? Taking equation (1),

$$\frac{d\alpha}{dt} = \kappa(\mathrm{A}-\alpha)(\mathrm{B}-\beta),$$

and supposing there is an excess of B sufficient to act on n times the amount of A present, then, as before, $\mathrm{B}=\nu n\mathrm{A}$, and also $\beta=\nu\alpha$. Since the amount of B rendered inactive is proportional to that of A up to any time, the above equation becomes

$$\frac{d\alpha}{dt} = \nu\kappa(\mathrm{A}-\alpha)(n\mathrm{A}-\alpha). \quad . \quad . \quad . \quad . \quad . \quad (5)$$

Since A and nA are the values of the active bodies before the action begins, they cannot be taken as the initial values when $t=0$, as the equation only applies when the change is actually going on, and the solutions require to stand a few minutes after mixing to allow the action to get into a normal state before the first observation can be made, when t is to be taken as $=0$. Putting $y=\mathrm{A}-\alpha$, or amount remaining at time t, the equation becomes

$$\frac{dy}{dt}=-\kappa\nu y((n-1)\mathrm{A}+y), \quad . \quad . \quad . \quad . \quad . \quad (6)$$

the solution of which is

$$\frac{1}{(n-1)\mathrm{A}}\log_\epsilon\frac{y}{(n-1)\mathrm{A}+y}=\mathrm{C}-\kappa\nu t \quad . \quad . \quad (7)$$

Let a be the value of y when $t=0$,

$$\mathrm{C}=\frac{1}{(n-1)\mathrm{A}}\log_\epsilon\frac{a}{(n-1)\mathrm{A}+a};$$

and inserting this value, the final equation becomes

$$-t=\mathrm{C}'\log_{10}\left\{\left(\frac{y}{(n-1)\mathrm{A}+y}\right)\left(\frac{(n-1)\mathrm{A}+a}{a}\right)\right\}, \quad . \quad (8)$$

where

$$\mathrm{C}'=\frac{\log_\epsilon 10}{\kappa\nu(n-1)\mathrm{A}}.$$

This equation is established on the same supposition as (4), and that the compounds formed during the action have little influence on the change either as retarding or accelerating agents, as indeed the experiments indicate.

Experiments.—As yet I have not been able to try very extreme values for n; in the experiments made it only ranged from ·5 to 5; above this latter value the action proceeded so rapidly that large errors occurred, rendering the results worthless: still I think the following experiments may be of interest as showing the truth of the formula.

The method of making the experiments was exactly the same as for those given in the first part of this paper.

Experiment (α).—The solutions employed were 25 cubic centims. ferrous sulphate (equal to ·4923 grm. iron), 10 cubic centims. potassic chlorate containing ·3593 grm., and 260 cubic centims. water—total volume 295 cubic centims,—this quantity of potassic chlorate being able to oxidize twice the amount of iron present, or $n=2$. The number of cubic centims. of permanganate required for 10 cubic centims. of this solution before the action commenced, or the value of A, was

determined by making a solution of iron containing ·4923 grm. Fe in 295 cubic centims. water and titrating 10 cubic centims. with the permanganate used in the experiment; its value was found to be 10·36 cubic centims., the values of y being, as before, the number of cubic centims. of permanganate required for 10 cubic centims. of the experimental solution.

The first observation, at $t=0$, gave $y=9{\cdot}45$ cubic centims.; and when $t=30{\cdot}5$ units, $y=7{\cdot}30$ cubic centims. Taking equation (8),

$$-t=\mathrm{C}'\log_{10}\left\{\left(\frac{y}{y+(n-1)\mathrm{A}}\right)\left(\frac{(n-1)\mathrm{A}+a}{a}\right)\right\},$$

and inserting those values, using the second observation to find C′, we get

$$-t=490{\cdot}2\log_{10}\left(\frac{y}{y+10{\cdot}36}\right)\left(\frac{19{\cdot}81}{9{\cdot}45}\right).$$

The values of t are calculated from this equation (in minutes), using the observed values of y, and are compared with those observed. The close agreement between theory and experiment is very striking.

Temperature 18° C. $n=2$.

Permanganate, in cubic centims.	Time, in minutes.	
Found.	Found.	Calculated.
9·45	0	
7·30	30·5	
5·98	55	56·4
4·74	89	89·1
4·06	112·2	112·6
3·30	143·2	144·8
2·63	180·5	182·5
2·30	206·8	205·5
1·93	237·5	236·5
1·58	272	273
1·14	336·3	334·4
·98	360	363·7

Experiment (β).—Every thing the same as before, except the ratio of the iron to the potassic chlorate, viz. 1 : 3, or $n=3$, and total volume 300 cubic centims. The value for A was found to be 10·18 cubic centims.; and taking the first two observations for a and C′, we get

$$-t=257{\cdot}4\log_{10}\left\{\left(\frac{y}{y+20{\cdot}36}\right)\left(\frac{30{\cdot}11}{9{\cdot}75}\right)\right\}.$$

The results are given below.

Temperature 18° C. $n=3$.

Permanganate, in cubic centims.	Time in minutes.	
Found.	Found.	Calculated.
9·75	0	
7·20	24	
5·23	50·3	51·4
3·73	81·8	82·5
2·86	106	108·1
2·06	139·3	140·8
1·46	172·8	172·6
1·15	199·5	201·3
·85	231	233·5
·61	264	269·4

Experiment (γ).—The ratio of the potassic chlorate to the iron was as 4 : 1, or $n=4$; and A was found by experiment to be 9·78 cubic centims. Using the first and third observation for a and C′, we get

$$-t=534{\cdot}1\log_{10}\left\{\left(\frac{y}{y+29{\cdot}34}\right)\left(\frac{38{\cdot}9}{9{\cdot}56}\right)\right\}.$$

Temperature 17°·8 C. $n=4$.

Permanganate in cubic centims.	Time, in minutes.	
Found.	Found.	Calculated.
9·56	0	
8·36	25·7	23·8
7·26	49·7	
6·38	75·1	74
5·56	104	100·5
4·86	128·3	127
4·32	152·5	150·7
3·86	175	173·6
3·07	217·5	221·1
2·68	249	249·8
2·22	293·5	290·2
1·98	317·5	315
1·71	346·5	346·9
1·35	402	399·1
1·30	410·5	407·4
1·1	447·5	444·7
·85	508	502·6

Experiment (δ).—In this case there was an excess of iron present, which alters the form of the equation on account of n being less than unity, the ratio of iron to potassic chlorate being as 1 : ·8, or $n=·8$; A was found to be 9·40 cubic centims. Taking the second and third observations for C,

and not the first for a, on account of its being faulty, we get

$$t-25{\cdot}6=2275{\cdot}7\log_{10}\left\{\left(\frac{y}{y-1{\cdot}88}\right)\left(\frac{6{\cdot}5}{8{\cdot}38}\right)\right\}.$$

Temperature 17°·8 C. $n={\cdot}8$.

Permanganate, in cubic centims.	Time, in minutes.	
Found.	Found.	Calculated.
9·17	0	1·3
8·38	25·6	
7·77	48·3	
7·20	74·6	73·5
6·68	102·8	101·1
6·30	126·8	124·8
5·96	151	149·1
5·70	173	170·1
5·26	216·5	211·6
4·95	248·5	246·8
4·64	292	288
4·48	315·5	312·2
4·26	345	349·2
3·98	401·5	406·4
3·88	438	429·5
3·57	508	513·6

The criterion of areas applied to those experiments gives very close results. The approximately true areas are given by the formula

$$\Sigma\left(\frac{y_{n+1}+y_n}{2}\right)(t_{n+1}-t_n);$$

and the real areas are found by assuming the curve to be

$$t=\mathrm{C}'\log_{10}\frac{ay}{b+y},$$

which gives for the area between the curve and axes, taking the limits for y of first and last experiment,

$$\text{area}=b\mathrm{C}'\log_{10}\left(\frac{b+y}{b+\mathrm{Y}}\right),$$

using the constants found by experiment.

Areas.

	Theory.	Found.	Percentage difference.
α.....	1230·4	1229·6	
β.....	823·4	819	·53
γ.....	1725	1744	1·11
δ.....	2716	2716·2	

In all the experiments given in this paper there is one special point to which I wish to draw attention; and that is that only two observations are taken to find the necessary constants; if these had been calculated for each pair of observations and the means taken, better agreement with theory would doubtless have been obtained; but such a procedure is apt to throw doubts on a theory, as to which, if it were true, one observation should be as good as another for determining the constants, unless large errors were suspected.

Before selecting those observations which were to be taken for the constants, the first few were laid off graphically, and those which seemed to be the most regular were chosen.

Influence of Temperature.—Chemical decompositions are all more or less influenced by heat, the effect being an acceleration of the rate of change; but it is very probable there is a point at or below which temperature no change can take place, as in the many cases where an acid has not action on a body at the ordinary temperature, while on heating to about 100° C. change goes on briskly.

The relation of the rate of change to the temperature and the point of zero action are possible to be discovered by the foregoing experiments in a simple manner.

It has been shown that the rate of change is expressed by the equation

$$\frac{dy}{dt} = -\mu y^2;$$

and if it be proportional to an unknown function of the temperature $f(\theta)$, θ being degrees Centigrade above the zero-point, so that

$$\frac{dy}{dt} = -\mu f(\theta) y^2,$$

integrating,

$$\frac{1}{\mu f(\theta)} = y\left(\frac{\mathrm{C}}{\mu f(\theta)} + t\right);$$

writing it in the usual form,

$$b = y(a + t),$$

b and a being determined experimentally. If for a second experiment under exactly the same conditions, but temperature differing by n° C., the equation is found to be

$$b' = y(a' + t),$$

we get at once the relation

$$\frac{b}{b'} = \frac{f(\theta + n)}{f(\theta)};$$

but it does not follow that $\frac{a}{a'}$ is also equal to this ratio, unless the value for y when $t=0$ is the same in each case.

A number of experiments were made to determine $f(\theta)$; but as the temperatures ranged only from 18° to 22° C., and they are rather incomplete, I refrain from giving them fully; the results, however, seemed to indicate that $f(\theta)=\theta^2$, or that the rate of change varies as the square of the temperature from the zero-point; and on this hypothesis the point of no action was found to range from $+2°$ C. to $-2°$ C., or about the temperature at which the solution would become ice.

In conclusion, I may state that the following experiments were made. An increase in the amount of H_2SO_4 accelerates the change, still obeying the law. Ferrous chloride, in presence of HCl, is oxidized by $KClO_3$, at 18° C., approximately according to the law $y(a+t)=b$, the discrepancies in this case being caused by the difficulty of accurately determining the iron by permanganate in presence of free hydric chloride. Ferrous sulphate in hydric sulphate is only very slowly acted on by potassic nitrate in the cold, probably being too near the zero-point.

I hope soon to give the results of some experiments on the rates of change for the various chlorates, as potassic against sodic chlorate, and possibly get some relation between the dynamical equivalences of those salts.

Glasgow, August 1878.

L. *Phenomena of Binaural Audition.*—Part II. *By* Silvanus P. Thompson, *D.Sc., B.A., Professor of Experimental Physics in University College, Bristol**.

1. In a paper read before Section A of the British Association last year (1877) on Binaural Audition†, the author communicated the discovery of two phenomena: first, the existence of an interference in the perception of sound; secondly, an apparent localization of simple sounds at the back of the head when led to the two ears in such a manner that the vibrations reached the ears simultaneously in opposite phases. The present paper recapitulates the former experiments, and gives some further account of the phenomena and of new methods of experimentation.

2. The existence of an interference in the perception of

* Communicated by the Author.

† Rep. Brit. Assoc. Plymouth, 1877, p. 37; Phil. Mag. October 1877, p. 274.

sounds was demonstrated by leading separately to the ears with india-rubber pipes the sounds of two tuning-forks struck in separate apartments, and tuned so as to "beat" with one another—the "beats" being very distinctly marked in the resultant sensation, although the two sounds had had no opportunity of mingling externally, or of acting jointly on any portion of the air-columns along which the sound travelled. The experiment succeeded even with vibrations of so little intensity as to be singly inaudible.

3. The apparent localization at the back of the head of sounds whose vibrations reach the ears in opposite phases being a subjective phenomenon, was announced by the author as the result of the concurrent testimony of several independent witnesses. Its existence was demonstrated by leading separately to the two ears the sound of two unison tuning-forks, one of which was slightly loaded to make its phase change slowly relatively to the other. The "beats" were described as being not "silences," as ordinarily observed when the difference of phase is half a complete vibration, yet as being "most distinctly heard, and seeming to be taking place within the cerebellum." Referring to this attempt to ascertain the effect of bringing to the two ears waves of equal pitch and intensity, but differing in phase, the author stated that a fuller series of experiments was in course of completion. The sequel of the present paper records with what result those experiments have been made.

4. The telephone of Graham Bell, introduced into this country at the Meeting at Plymouth, where the former paper was read, furnishes a new instrument peculiarly adapted for researches of this nature. Both the phenomena recapitulated above have been reobserved by a considerable number of independent experimenters, confirming the results announced by the author. Thus it was announced by Professor Graham Bell, in a lecture before the Society of Arts, November 28, 1877*, that these two phenomena had been discovered by Sir William Thomson when experimenting at Glasgow with the telephone.

One important advantage of the Bell telephone as an instrument of research is that the phase of vibration can be inverted at will, by reversing the connexions of the receiving-instrument, so that the currents traverse the coil in a reversed direction, the motion of the vibrating disk being consequently also executed in a reverse sense. Using two systems of telephones to bring to the ears two sounds capable of yielding interference-beats, interference in the resulting perception of the sounds is

* *Vide* Journal of the Society of Arts, vol. xxvi. No. 1306, p. 22 (Nov. 30, 1877).

very clearly recognized. I have not been able hitherto, however, to devise any crucial experiment to decide whether the interference is an interference of the sensations, or whether it is to be attributed to the physical conveyance of the sounds through the bones of the skull, and their mechanical interference.

5. *Phenomena of Localization.*—Almost all persons who have experimented with the Bell telephone, when using a pair of instruments to receive the sounds, one applied to each ear, have at some time or other noticed the apparent localization of the sounds of the telephone at the back of the head. Few, however, seemed to be aware that this was the result of either reversed order in the connexion of the terminals of the instrument with the circuit, or reversed order in the polarity of the magnet of one of the receiving-instruments. When the two vibrating disks execute similar vibrations, both advancing or both receding at once, the sound is heard as usual in the ears; but if the action of one instrument be reversed, so that when one disk advances the other recedes, and the vibrations have opposite phases, the sound apparently changes its place from the interior of the ear, and is heard as if proceeding from the back of the head, or, as I would say, from the top of the cerebellum. So distinctly marked is the apparent localization, that it has been regularly employed, I am informed, by Professor D. E. Hughes to ascertain whether a pair of receiving-telephones are rightly adjusted or not.

6. My recent experiments have been directed to determining how this apparent localization is affected by variations of the sounds in respect of (*a*) pitch, (*b*) phase, (*c*) intensity, (*d*) quality, and whether it is to be accounted as a physical, physiological, or psychological phenomenon. I have made, finally, a few experiments on the binaural estimation of combinational tones.

(*a*) After employing the simple sounds of tuning-forks of various pitches, and conveying their vibrations to the two ears in opposite phases by three distinct methods, I find the apparent locality of the sound (the acoustic "image") to occupy an invariable position near the top of the cerebellum. These three methods are:—*First*, employing two india-rubber tubes of equal length, armed with either glass funnels or box resonators, in front of which two tuning-forks are held, difference of phase being obtained by rotating one fork round on its axis, or by loading it to obtain a continuously varying phase. *Secondly*, by employing one tuning-fork, but having a branching tube to the ears, and making the lengths of the two branches differ by half a wave-length. *Thirdly*, by em-

ploying a system of telephones, the two receiving-telephones being reversed in action.

For simple tones, then, the acoustic "image" does not vary in position with pitch, if the difference of phase remains always a maximum. For compound tones the first and second of these methods of experiment are impossible; but when using the two telephones, I obtain the localization in the same position for sounds of all pitches and kinds. Indeed the observation of articulate speech appearing to come from a spot at the back of the head proves the phenomenon to be independent of the pitch—that is to say, of the wave-length of the component tones.

I arranged a Hughes's microphone with two cells of Fuller's battery and two Bell telephones, one of them having a commutator under my control. Placing the telephones to my ears, I requested my assistant to tap on the wooden support of the microphone. The result was deafening. I felt as if simultaneous blows had been given to the tympana of my ears. But on reversing the current through one telephone, I experienced a sensation only to be described as of some one tapping with a hammer *on the back of the skull from the inside.*

7. The relation of the acoustic "image" to the difference of phase (b) between the two separate sounds is a more difficult matter to deal with. The method of the telephone here becomes inapplicable, as it provides no means of partially changing the phase of one of the two sounds. The two sounds must be either exactly coincident or exactly opposed in phase. With simple tones, as of the tuning-fork, any desired difference of phase can be obtained by adjusting to the appropriate amount the difference in length between the two conveying tubes; or by loading one of the two forks so as to make its vibrations lose gradually on those of the other fork, and so establish a regularly changing difference of phase. Both these methods agree in showing, as far as accurate observation of the subjective phenomenon is possible, that when the difference of phase is partial, the sound is heard partly in the ears and partly at the back of the head, the former partial sound dying out and giving place to the latter as the difference of phase approaches a maximum, and *vice versâ.*

A variation of this experiment may be of interest to record. A stout copper wire about 3 feet long had its ends bent round into smooth loops, and the whole was curved so that the two loops could be inserted into the two auditory meatus of the ears. An UT_3 ($c' = 256$) tuning-fork was then struck; and its stem was pressed against the middle point of the copper wire, the vibrations having thus to travel equal lengths of the wire

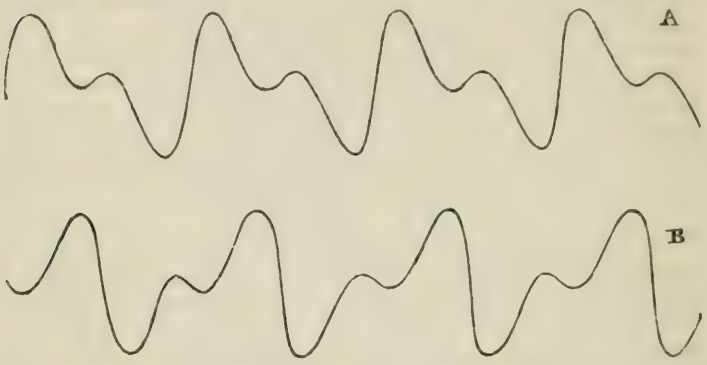
before reaching the ears. The sound appeared to come from the ends of the wire in the ears. The stem of the fork was now slid along the wire. At about an inch and a third from its former position the difference of path of the two sounds had been sufficient to produce complete difference of phase, and the sound appeared to come, not from the two ends of the wire, but from the back of the head. In intermediate positions the effect was of a mixed character: part of the sound was heard as in the ears, part at the back of the head. With forks of various pitches, a similar result was found, with appropriate differences in the lengths of path.

8. The next experiments concern the relative intensities (*c*) of the two sounds. When two simple tones in unison and agreeing in phase are led to the ears, one tone being louder than the other, the simple result of experience is that the sound is heard in one ear more than in the other. It is as if the sounds were loud, but as if one ear were partly deaf. But if the two simple sounds differ by exactly half a vibration, and one is louder than the other, the result is wholly different. The sound no longer seems to be in the ears. There is an acoustic "image" localized at the back of the head. Instead, however, of this image appearing to exist at the centre of the back of the head as it does when the sounds are equal in intensity, it appears to be on one side, nearer the ear in which the sound is louder. If two unison tones reach the two ears separately, with complete difference of phase and equal intensity, and then the intensity of one sound be gradually reduced down to nothing, the acoustic image, which at first occupies a position behind the middle of the top of the cerebellum, gradually moves round the back of the head apparently just within the skull, to the ear in which the sound arrives with full intensity. I have verified this result with tuning-forks and tubes, and also with the microphone and two Bell telephones, in one of which the intensity of the vibration could be regulated by adjusting the position of the magnet.

9. The effect of the *quality* (*d*) of a compound sound on the localization of an acoustic image in binaural audition is very complicated. The action of the telephone in reversing the phase of *all* vibrations independent of their quality has already been noted in § 6.

A case, however, occurred, of some independent interest. Suppose a simple tone and its octave to be combined together, so yielding a compound wave of definite form. Suppose now the octave note to be simply reversed, and in this condition compounded with the original fundamental tone. The form of the second compound wave will exactly resemble that of

the first, except that it is (so to speak) inverted end for end. The two associated wave-forms might be, for example, the two (A and B) subjoined. The question arises, Can we dis-



tinguish between the two compound tones corresponding to these forms by leading them separately to the two ears—that is, by listening to them *binaurally*? We have shown that in binaural audition we have a perception of a difference of phase for single tones. Can we go further, and demonstrate the existence of a perception of phase-difference in the components of a compound sound?

Helmholtz*, after a very careful consideration of the subject, comes to the conclusion that in ordinary hearing "*the quality* of the musical portion of a compound tone depends solely on the number and relative strength of its partial simple tones, *and in no respect on their difference of phase.*" Mr. Sedley Taylor† repeats the statement in a more emphatic form: "The ear being deaf to differences of *phase* in partial-tones, perceives no distinction between such modes of vibration . . . . but merely resolves them into the same single pair of partial-tones."

To put the matter to the test of binaural hearing, the following experiment was arranged:—Two similar glass funnels were attached to equal tubes leading to the two ears: their mouths were placed facing one point, but at right angles to one another. If a vibrating tuning-fork be held at the point toward which the funnels face, it can be so held that the vibrations imparted by the two tubes shall be either alike or opposite in phase. For if the ends of the prongs be held towards one funnel so that the axis of the fork coincides with the axis of

* Sensations of Tone (Ellis's translation), p. 184.
† Sound and Music, p. 146.

the funnel, and then the fork be slowly rotated round its axis, the vibrations transmitted down the other funnel will alternately agree with or differ from those transmitted down the first funnel, according to the aspect of the fork. Two forks were chosen for the experiment, UT_3 and UT_4 ($c' = 256$ and $c'' = 512$). Both were set vibrating at the point whence their vibrations could enter each funnel; and they were placed at first so that the vibrations of each fork reached the ears in similar phases. The compound tone was heard "in the ears." Then the UT_4 fork was rotated a quarter round on its axis, so that it sent opposite vibrations to the two ears. The two compound waves transmitted by the two tubes differed therefore like those of the diagram above. *The sound of the* UT_4 *fork moved its position to the back of the head; the sound of the* UT_3 *fork remained localized in the ears.* When the UT_3 fork alone was turned round a quarter, its sound alone was localized at the back of the head. When both were turned one quarter, both notes appeared to sound at the back of the head. Here is a case, then, in which by listening with the two ears we can detect a difference of phase between the component partial tones of a compound tone. And when we reflect that, except for sounds whose origin is in the plane coinciding with the median plane of the skull, the lengths of the paths by which a sound reaches the two ears are unequal, so that the phases of the vibrations received simultaneously by the two ears cannot be alike for all wave-lengths, we must admit that this binaural perception of a difference of phase cannot but be of importance in the appreciation by the ear of the quality of compound sounds such as those of musical instruments (in which every fundamental tone is accompanied by a certain characteristic series of upper partials), and of the sounds of the vowels.

10. As to the cause of the singular localization discussed in the preceding paragraphs, whether it be physical, physiological, or psychological (that is to say, purely associative), I hesitate to give an opinion without further evidence. In searching for a true explanation we must not lose sight of the other phenomenon of the interference in the perception of sounds, though it is by no means certain that both phenomena can be referred to the same cause. There is no decussation of the auditory nerves, like that of the optic nerves, to account for a blending of the sensations. The *portio mollis* of the right does not intersect or have any commissure with the *portio mollis* of the left after leaving the fourth ventricle of the brain, from which they originate. This point deserves the attention of anatomists and physiologists.

It may be possible to explain the phenomenon of localiza-

tion upon a hypothesis of a pure association of previous sensations; but such cannot be framed until the physiological points involved are more distinctly recognized.

11. *The Binaural Estimation of Combinational Tones.*—In my paper of last year I remarked that I had been unable to observe the existence of combinational (differential) tones when two simple tones ordinarily capable of yielding such tones were led separately to the two ears. Thus the forks e' and g' (MI_3 and SOL_3) when struck together give a differential tone of 64 vibrations, or $C_,(=UT_,)$, which is perfectly recognizable; but when led separately to the ears no such tone is to be heard. But if the two tones be allowed to mingle and their joint sound be led to the two ears by a Hughes's transmitter and two telephones arranged to yield opposite vibrations, then the differential tone of 64 vibrations is heard accompanying the two simple tones, and is localized with them at the back of the head.

Other experiments with forks of higher pitch have convinced me that the absence of differential tones is general when the two separate simple sounds do not mingle before reaching the ears. During a recent visit to the workshop of Dr. R. König of Paris, I had the opportunity of hastily repeating this experiment upon forks of shrill tones belonging to a series purposely constructed for observations upon the combinational tones; and the result fully confirmed previous observations.

My paper of last year mentioned a case which seemed to indicate the presence of summational tones. The difficulty of observing these is ordinarily much greater than that of observing the differential tone, and in binaural experiments is still greater. Further observations are still wanting on this point. It was remarked in my former paper that in binaural audition dissonances are excessively disagreeable, and the ordinary consonant intervals harsh. No subsequent experiments have been made upon this matter, which is probably connected with the absence of the differential tones.

12. One other phenomenon, as yet only partially investigated, remains to be recorded. Let a small tuning-fork, such as that used by pianoforte-tuners or violin-players, be struck, and its stem pressed against any portion of the back of the head. If the fork be pressed against the parietal or occipital regions of the head on the right of the median line, its sound appears to be heard in the left ear; if held against a region of the head on the left, it appears to be heard in the right ear. If, however, the fork be pressed against the region immediately above either temporal bone, its sound is heard in the ear of the same side.

Recapitulation.

(*a*) There is an interference in the perception of sound; for two simple tones capable of interfering are still heard to interfere when conducted separately to the two ears.

(*b*) When two simple tones in unison reach the ears in opposite phases, the sensation of the sound is localized at the back of the head.

(*c*) The localization of this acoustic "image" is independent of the pitch of the sounds.

(*d*) When the difference of phase is partial, the sensation is localized partly in the ears and partly at the back of the head.

(*e*) If the difference of phase be complete but the intensities unequal, the acoustic "image," instead of being at the middle of the back of the head, is nearer that ear in which the sound is louder.

(*f*) It is possible to discern the difference between two compound tones which differ only in the phase but not in the pitch or intensity of their component partial tones. For when two such compound tones are separately brought to the ears so that the vibrations of any partial tone present reach the ears in opposite phases, that particular partial tone is singled out and localized at the back of the head.

(*g*) When two simple tones are led singly to the ears no differential tone is heard; there is some evidence that summational tones are heard.

(*h*) To binaural audition dissonances are excessively disagreeable, and ordinary consonances harsh.

(*i*) Vibrations mechanically conveyed to a point of the parietal or occipital region of the skull, at one side, are apparently heard in the ear of the other side of the head.

University College, Bristol,
July 30, 1878.

LI. *Notices respecting New Books.*

A Treatise on Dynamics of a Particle, with numerous Examples. By Peter Guthrie Tait, *M.A., formerly Fellow of St. Peter's College, Cambridge, Professor of Natural Philosophy in the University of Edinburgh, and the late* William John Steele, *B.A., Fellow of St. Peter's College, Cambridge.* Fourth Edition, carefully revised. London: Macmillan and Co. 1878. (Crown 8vo, pp. 407.)

THIS work has been long before the world, and its merits as a comprehensive handbook of the subject are well known. In fact it contains an account of the Motion of a particle under various circumstances amply sufficient for the requirements of the most ad-

vanced student. After two introductory chapters (one kinematical, the other physical), there are chapters devoted to rectilinear and to parabolic motion, to central orbits, to constrained motion and to motion in a resisting medium: then follow a chapter of "General Theorems," one on impact, and, finally, a chapter on the motion of two or more particles. The last of these subjects is treated under the two divisions of free motion and constrained motion. In the second division no more than the case of two particles is considered. The methods employed "are applicable to more complicated cases, when more particles than two are involved: but nothing would be gained by such a proceeding, as D'Alembert's Principle supplies us with a far simpler mode of investigating the motions of any system of free or connected particles" (p. 370). In fact the only case discussed in detail is that in which two particles of unequal masses "are attached to different points of an inextensible string, one of whose extremities is fixed." The solution of this question is obtained for small vibrations, in which case the integrations can be effected in finite terms. There is added a discussion of the case in which the mass of one particle is much greater than that of the other, and the strings are not approximately equal. This appendix is taken from a paper by Sir W. Thomson, "On the rate of a Clock or Chronometer as influenced by the mode of Suspension." To each chapter numerous examples are appended, which have been taken for the most part from Cambridge Examination Papers; many of them would require for their solution an amount of ingenuity such as but few students possess.

Professor Tait gives by way of introduction a short history of the book, from which it appears that Mr. Steele's share in the composition of the First Edition was not very large, and, in fact, that he died before its final arrangement and revision. The Second Edition was thoroughly revised and, indeed, recast by Professor Tait, who thus appears to be the sole responsible author. The Third Edition was revised by W. D. Niven, Esq., of Trinity. "The present Edition has been thoroughly revised by Professor *Greenhill*, who has not only at great labour verified and (where necessary) corrected the examples, but has endeavoured to adapt the book to the present requirement of the Tripos by the free introduction of Elliptic Functions, &c., which, in my Cambridge days, were under the ban of the *Board of Mathematical Studies*." In short, it will be seen that no pains have been spared to render the work as complete and trustworthy as possible. And yet the author does not seem very well satisfied with his work. The fact is that the book was written with a view to the requirements of the Mathematical Tripos; and this circumstance seems to have imposed restraints at which Professor Tait chafes. Thus we find him writing as follows:—Several of the examples "have defied all attempts at improvement, and now stand in their unintelligibility as a warning, to the Candidate for Mathematical Honours, of the ordeal he may have to pass through." Again, the text of the work is to a great

extent broken up into detached propositions, yet he states that in his opinion "this is not the form in which such a treatise ought to be written" (pp. vi, vii). And with reference to the chapter of "General Theorems," he says that several of the results proved in it "have already occurred as immediate deductions from the laws of motion; but to maintain the special character of the work we give more formal analytical demonstrations, though these are certainly superfluous" (p. 259). We cannot help thinking that the unintelligible examples should have been omitted, and that the book should have been written in the form that seemed best to the writer. But if this could not be, and he were in any sense writing to order and so obliged to compromise, surely it is somewhat ungracious to proclaim his dissent, from what is after all his own act and deed, upon the house-tops. The fact is, if we may venture to hint at a fault, the Professor's individuality is a little too pronounced. If he thought it best, on the whole, not to act upon his private opinion, it would have been better to have kept silence. Occasionally the fault takes another form, and he indulges in some thing that might almost be called autobiography. A very curious instance of this is to be found in the confession that, when he wrote the second chapter as it stood in the First Edition, he had not so much as read "Newton's admirable introduction to the *Principia*."

LII. *Intelligence and Miscellaneous Articles.*

A CONSIDERATION REGARDING THE PROPER MOTION OF THE SUN IN SPACE. BY S. TOLVER PRESTON.

IT is a known fact that a wave emitted in a medium does not partake of the motion of the body emitting it; for when once the wave has left the body, the wave depends solely on the medium for its propagation. Hence it would follow that, owing to the sun's proper motion in space, the waves emitted by the sun must be situated *excentrically* about it, the degree of excentricity marking exactly the direction and velocity of the sun's proper motion in space (or in the æther which fills all space). It thereby becomes possible (in imagination at least) to refer the proper motions of the sun and stars to one common standard, viz. to the universally diffused æther: or it may be said that the direction and velocity of the sun's proper motion (and that of every star) is physically *marked* in the æther of space—each stellar sun marking the direction and velocity of its motion with geometrical accuracy by means of the relative situation to one another of the spherical waves successively emitted, each of which remains immovable or unalterable in position (in regard to its own centre). In short the centre of a spherical wave may be said to represent *indestructible position*, in the sense that the centre of the spherical wave is an immovably fixed point, such that the intersection of the normals to two tangent planes situated anywhere on the contour of the spherical wave

always marks the same indestructible locality (which is entirely unaffected by any subsequent motion of the body which emitted the wave). In this sense it may be said to be theoretically true that the locus or origin of any disturbance in a medium is for ever defined in that medium.

If we imagine a boat sailing in a smooth lake, and drop periodically stones into the water, circular waves will spread each time from the centre of disturbance, and the degree of excentricity of these waves to the moving boat (or to each other) will mark geometrically the direction and velocity of the boat's motion. So the degree of excentricity of the waves about the moving sun (set up by the disturbing impulses of its molecules) marks geometrically the direction and velocity of the sun's proper motion.

The following point in connexion with this subject may perhaps be worth a passing notice. It has been computed (according to an estimate of Sir William Thomson, based on the observations of Herschel and Pouillet), that the energy of the waves given off by the sun amounts (in round numbers) to 7000 horse-power per square foot of surface; or, otherwise, about 1700 foot-tons of energy are thrown off per second from every square foot of the sun's surface into the æther of space. It would seem incredible (whatever the constitution of the æther might be imagined to be) that all this energy could be given off to a material medium in a particular direction (*i. e.* in a direction *from* the sun) without any reaction in the opposite direction: or would it be supposed that, if the sun were emitting all this energy from *one side only*, there would be no reaction in the opposite direction? Admitting that there would be a certain reaction in this assumed case, then it would be reasonable to conclude that, as the case actually stands, the reaction would not be *perfectly* balanced, owing to accidental irregularities in the distribution of the differently radiating materials of the sun's surface. If this be admitted as possible, then we should have in the unbalanced reaction a true physical cause for producing (or influencing) the proper motion of the sun.

London, October, 1878.

ON THE DISSOCIATION OF THE OXIDES OF THE PLATINUM GROUP.

BY H. SAINTE-CLAIRE DEVILLE AND H. DEBRAY.

Platinum is distinguished from all the metals with which it is associated in its ores by the fact that it does not combine directly with oxygen, in whatever position we place the two bodies.

With rhodium, palladium, and iridium the case is different. When heated in a muffle, if the temperature is not too high, these metals combine with oxygen; but their oxides are decomposed when we raise the temperature sufficiently.

Osmium and ruthenium combine directly with oxygen. The product of this oxidation is volatile, and is formed at the highest temperatures.

The most strongly calcined osmium, much less alterable than the osmium obtained at a low temperature, changes into osmic acid at ordinary temperature*; at the highest temperatures osmic acid is always obtained.

Ruthenium behaves altogether like osmium. The strongly calcined metal oxidizes in a muffle at a temperature scarcely above 400° C., and volatilizes in great quantity. In what form, it is difficult to say; for the material then exhales the odour of ozone, the formation of which always accompanies the decomposition of hyperruthenic acid. The product deposited in the muffle is always the binoxide of ruthenium. This oxide sublimates and crystallizes in a porcelain tube traversed by a current of oxygen in which substances containing ruthenium are heated. M. Frémy thus proved its volatility, and discovered the means of extracting directly, and under the most beautiful forms, some products of the roasting of osmide of iridium. Ruthenium also, which in the metallic state is one of the most fixed substances we know, evaporates very quickly in a muffle, especially at a very elevated temperature†.

These properties absolutely distinguish osmium and ruthenium

* The characteristic smell of osmic acid is at length perceptible in bottles containing even crystallized osmium prepared at a high temperature. The stoppers then become covered with the black coating given by the reduction of osmic acid.

† By operating on a few grams of ruthenium we have been able in a few hours to volatilize 24 per cent. of its weight in a highly heated muffle; in the blowpipe flame the volatilization is much more rapid and still more considerable in amount.

From the fact that osmium and ruthenium volatilize very rapidly in the oxyhydrogen flame of the blowpipe, giving osmic acid and binoxide of ruthenium, it must not be concluded that these oxides are undecomposable by heat. Osmic acid would be reduced, in the interior of the flame at that high temperature, to sesquioxide of osmium, crystallizable in golden-coloured scales, which we have made known. The final result would still be the same: this oxide, on coming into the air, into a relatively cold region, would there be changed into osmic acid. In supposing that the oxide of ruthenium is not decomposed into a lower oxide, it is not necessary also to admit that it is absolutely undecomposable at the temperature of 2500° given by the combustion of hydrogen and oxygen. This combustion is not complete in the hottest parts of the flame; hydrogen and oxygen exist there uncombined; and if the oxide of ruthenium has a less tension of dissociation than that of water at this high temperature, one can conceive that it may not be decomposed. Oxide of ruthenium would therefore be less readily decomposable than water by heat. To discover whether it is really undecomposable, we must be able to heat it (as we have pointed out for the oxide of iridium) in a vacuum to high temperatures; but, unfortunately, there exist no vessels suitable for such experiments. The hypothesis of the very great stability of the oxide of ruthenium, however, is supported by the fact that this oxide, heated in a porcelain tube, exhibits (like the oxide of iridium), at least at a bright red heat, no sensible tension of dissociation; it merely volatilizes, and is deposited in the form of crystals and a coating of binoxide in the colder parts of the porcelain tube.

from the other metals of the platinum group. These two bodies, by the manner in which they behave in contact with oxygen, manifestly approach arsenic and antimony; they might, like these, be placed among the metalloids. It is not the same, we have said, with rhodium, palladium, and iridium. These bodies once oxidized, decomposing by heat, permit us to ascertain the laws of their dissociation and the tension it takes at different temperatures. We will only speak, in this Note, of the oxide of iridium, the only one we have at present completely studied.

The oxide is put into a porcelain tray, this into a small platinum waggon, which is introduced into a porcelain tube closed at one end by a glass plate kept in its place by mastic. The other extremity, by means of a lead pipe and a glass tube, attached one to the other by mastic and to the porcelain tube, is put into communication with a Geissler mercury pump, and a manometric tube dipping in the mercury.

Before introducing the iridium oxide into the porcelain tube, we assure ourselves that it maintains a vacuum at the ordinary temperature and at a red heat, which does not always happen. In fact, porcelain tubes which are air-tight when cold, at a red heat often let in hydrogen and carbonic oxide from the hearth.

The porcelain tube is introduced into a cylindrical muffle capable of containing at the same time a porcelain thermometer furnished with its tube and compensator, of the form employed and described by M. Troost and one of us*. The muffle is placed in a furnace heated with petroleum or with heavy coal-oil, which is introduced into the furnace through a piston-cock divided into at least 200 parts; so that the flow of the oil can be varied, and consequently the temperature, with a perfection that could not have been expected. The oil-reservoir is furnished with a Mariotte tube, which maintains it at a constant pressure. With this apparatus the porcelain can be completely fused.

We commence by heating the muffle to the point at which the tension of the liberated oxygen is from 30 to 40 centims. and returns to the same when the apparatus has been several times exhausted by means of the Geissler pump. We are then sure that the composition of the undecomposed oxide of iridium no longer varies. Then with the cock we lessen the flow of the coal-oil until the pressure of the oxygen does not exceed a few millims. and remains constant. This is noted, and the temperature determined.

We then successively augment the flow of the oil in order to obtain higher temperaiures and stronger tensions of dissociation; these are noted when they have become constant. Thus the following numbers were found:—

* This apparatus has been simplified by employing a Sprengel pump, which permits the thermometric material (nitrogen) contained in the reservoir to be taken out and measured as often as we wish, and the temperature to be calculated.

Temperatures.	Dissociation-tensions. millim.
°	
822·8	5
1003·3	203·27
1112·0	710·69
1139·0	745·00

If, after a determined temperature and tension have been attained, oxygen is withdrawn by means of the Geissler pump, the mercury returns to the initial tension, provided, of course, undecomposed oxide of iridium remains. The dissociation-tension of this oxide depends, then, solely on the temperature.

If the temperature be raised above 1139°, the dissociation-tension soon exceeding that of the atmosphere, oxygen is rapidly liberated through the mercury; when this has quite ceased, we exhaust; and on taking out the tray containing the oxide from the cooled tube, we find in it some metallic iridium, consequently reduced by the mere action of heat.

The tension of oxygen in air being about 152 millims., it follows from the above-cited numbers that the oxide of iridium decomposes in the open air at a temperature below 1003°·3, and consequently that at that temperature, or at any higher one, iridium is absolutely non-oxidizable in the air.

When we break the porcelain tube in which the oxide was heated, we observe that it is covered, in the parts which were but little heated, with a very thin layer of blue oxide of iridium—which demonstrates a slight volatility of this oxide at the relatively low temperatures at which it can exist. Above 1000° volatilization becomes impossible in our atmosphere, since the oxide of iridium ceases to exist there and the metal is at least as fixed as platinum. We have likewise proved this feeble volatility of the oxide of iridium in other experiments, made with M. Stas.—*Comptes Rendus de l'Académie des Sciences,* Sept. 23, 1878, tome lxxxvii. pp. 441–445.

ON A UNIVERSAL LAW RESPECTING THE DILATATION OF BODIES.

BY M. LÉVY.

Between the specific volume of a body, its temperature, and the pressure (supposed normal) which it supports, there exists, as is known, a relation which permits one of these three quantities to be expressed as a function of the other two—for example, the pressure as a function of the volume and temperature.

Hitherto (to my knowledge at least) theory has supplied no indication upon the nature of this relation, and nothing permits us to affirm with certainty that it may not change in some way when we pass from one substance to another. For every substance the physicist is condemned to seek it in every particular by experiment, which, in a manner, necessitates ∞^2 number of observations.

I purpose to demonstrate that this relation is far from being possibly arbitrary—that *the pressure supported by any body whatever, so long as the body does not change its state, can only be a linear function of its temperature.* In other terms, under a physical form, *If any body whatever be heated under a constant volume, the pressure which it exerts upon the immovable sides of the enclosure which contains it cannot but increase in rigorously exact proportion to its temperature.*

I say that this proposition is an *absolutely rigorous* corollary of the two fundamental propositions of the mechanical theory of heat, and of the hypothesis that the reciprocal actions of the atoms of bodies are directed along the lines which join their points of application, and depend only on the distances of these points from one another.

To demonstrate the law above enunciated, let dQ be the quantity of heat necessary in order to modify infinitesimally the volume v, the pressure p, and the temperature T of a body without its changing its state. The first principle of the mechanical theory of heat gives the classical equation

$$dQ = dU + Ap\,dv, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

$A = \frac{1}{E}$ being the thermal equivalent of the work, and U the function which is often called the *internal heat.* Let us take v and T for the independent variables, so that

$$dU = \frac{dU}{dT}\,dT + \frac{dU}{dv}\,dv.$$

The signification of each of the two terms of the second member is obvious: the first represents the quantity of heat necessary to produce an increment dT of the temperature without change of volume; consequently, and since there is no change of state, the second necessarily represents the quantity of heat equivalent to the work of the molecular actions during the increase of volume dv. Now, if we represent by $mm'f(r)$ the amount of the mutual action of two molecules the masses of which are m and m', placed at the distance r the one from the other, this work is represented by an expression of the form $\Sigma mm'f(r)dr$, so that we have identically

$$\Sigma mm'f(r)dr = E\frac{dU}{dv}\,dv.$$

The first member not containing the letter T, it is the same with the second. Therefore $\frac{dU}{dv}$ depends only on the variable v alone; and consequently U is of the form $F(T)+f(v)$. Hence this first consequence:—*The internal heat of a body cannot be any function of the specific volume and the temperature of that body; it can only be the sum of two functions,—the one, of the volume alone; the other, of the temperature alone.*

This first corollary also results immediately from the statement, so luminous in its brevity, of the mechanical theory of heat, given by M. Resal in his *Mécanique générale.*

We can therefore write

$$dU = A[T\phi'(T)dT + Rdv],$$

R being a function of v only, and $\phi'(T)$ any function of the temperature.

Observe now that, in virtue of the second general proposition of the mechanical theory, we have, if T is the absolute temperature and μ denotes what M. Clausius calls the *entropy*,

$$dQ = Td\mu = T\left(\frac{d\mu}{dT}dT + \frac{d\mu}{dv}dv\right).$$

Introducing these values of dU and dQ into equation (1), we get

$$T\left[\frac{d\mu}{dT} - A\phi'(T)\right]dT + \left(T\frac{d\mu}{dv} - AR - Ap\right)dv = 0,$$

which requires that we have separately

$$\frac{d\mu}{dT} - A\phi'(T) = 0,$$

$$T\frac{d\mu}{dv} - AR = Ap = 0.$$

From the first we derive, V being an arbitrary function of the variable v alone,

$$\mu = A\left[\phi(T) + \int\frac{dv}{V}\right].$$

We thus get this second proposition:—

Whatever the body considered, the quantity called by M. Clausius the entropy *cannot be any function of the volume and the temperature; in the same way as the internal heat, the entropy can only be the sum of two functions,—the one, of the volume alone; the other, of the temperature alone.*

Consequently the second of the equations obtained gives

$$(p + R)V = T, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (A)$$

which establishes the law above enunciated. Such is the *necessary* form which connects the pressure, the volume, and the temperature of any body whatever, R and V being two functions of v only.

We said, at the commencement, that theory had not hitherto supplied any certain and general indication like that which is in question here. On this subject we must make one remark. M. Hirn, in the last edition of his *Exposition de la Théorie mécanique de la chaleur*, very judiciously divides that theory into two branches: in the first he develops the *rigorous* consequences of the two fundamental propositions; in the second he states a great number of

philosophical views and intuitive results. In the second part, particularly, M. Hirn points out how one might, in his opinion, render the laws of Mariotte and Gay-Lussac applicable to all bodies, on condition of adjoining to the pressure p, which enters into them, a fictitious pressure R equivalent to what he calls the *sum* of all the molecular actions, which would not be dependent on the volume. He thus arrives at the formula

$$(p+\mathrm{R})\frac{v}{\mathrm{T}}=\mathrm{K}=\text{constant},$$

coinciding with the laws of Mariotte and Gay-Lussac for $\mathrm{R}=0$.

It will be seen that this formula and our (A) would be identical if it were admitted that, for all bodies, the function V, introduced by our analysis, follows a simple law of proportionality. It is very remarkable that analysis thus confirms, not in whole, but at least in part, the results to which M. Hirn was led by the profound meditations developed by him in what he calls the second part of thermodynamics, the part which might be named philosophic and speculative. Our law (A), provided the fundamental hypothesis of molecular mechanics be admitted, must evidently be ranged in the first, the rigorous part.—*Comptes Rendus de l'Académie des Sciences*, Sept. 23, 1878, tome lxxxvii. pp. 449–452.

ON DIFFUSION AS A MEANS FOR CONVERTING NORMAL-TEMPERATURE HEAT INTO WORK.

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

In the Philosophical Magazine for September last is a translation of a paper* by Professor Clausius, replying to an article of mine on the subject of the work to be derived at the expense of normal-temperature heat through diffusion†. After expressing his agreement with the general principle involved, Professor Clausius states, "only on one point I think I must express a view different from his." Will you allow me to express my thanks to Professor Clausius for his kindly criticism, and, in justice to myself, to point out that I had already seen my mistake and retracted it‡.

I am, Gentlemen,

Yours faithfully

London, October 1878. S. Tolver Preston.

* Wiedemann's *Annalen*, July 1878, p. 341.

† 'Nature,' Nov. 8, 1877, p. 31; and Jan. 10, 1878, p. 202.

‡ 'Nature,' May 23, 1878, p. 92.

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[FIFTH SERIES.]

DECEMBER 1878.

LIII. *On the Mechanical Theory of Crookes's (or Polarization) Stress in Gases.* By G. JOHNSTONE STONEY, *M.A., F.R.S., &c., Secretary to the Royal Dublin Society**.

Introduction.

TWO papers will be found in the first volume of the fifth series of the Philosophical Magazine (March and April 1876), in which I endeavoured to explain the force that Mr. Crookes had detected within vacuum-chambers, by pointing out that when heat passes across the residual gas, the molecules of the gas that tend respectively towards the heater and towards the cooler must interpenetrate one another in a greater degree than they would if the gas were in its ordinary or unpolarized condition, and that this behaviour will render the stresses within the gas unequal, causing the stress to be greatest in the direction in which the augmented interpenetration takes place.

When writing the foregoing papers, and afterwards when writing a paper on the transfer of heat which accompanies the phenomenon, I was under the mistaken impression that the flow of heat between a heater and cooler in fixed positions, and at constant temperatures, will become greater if the number of gaseous molecules that intervene is reduced below the number required for the transfer of heat by the laws of "con-

* From the 'Scientific Transactions of the Royal Dublin Society' for the year 1878. Read February 18, 1878. Communicated by the Author.

duction"*; and for this supposed increased flow of heat I suggested the name penetration. It has recently been pointed out by Dr. Schuster ('Nature,' vol. xvii. p. 143) that experiments have been made which show that the flow of heat diminishes instead of increasing when the limit for "conduction" is passed. It thus appears that what I have called penetration is always feebler than conduction, and is to be sought, in the figures representing De la Provostaye and Desains's experiments, in those portions of the curves which slope steeply downwards. Accordingly my paper on Penetration (Phil. Mag. December 1877), and especially that part of it in which I apply the theory to experiment, requires considerable modification, and some of the statements I made in my earlier papers on Crookes's force need amendment. Although the corrections that are required do not affect any material part of the theory of unequal stresses within polarized gas, it has appeared desirable to resume the subject and present the theory freed from the error that has been pointed out. In doing this I have taken the opportunity of introducing the conception of the *reflecting tube*, which greatly facilitates the inquiry into the mechanical effect of the interpenetration; and I have also availed myself of the admirable method of treating the problem, described by Mr. George F. Fitzgerald in 'Nature,' vol. xvii. p. 200, to obtain a complete expression for the stress, and to show that my theory is not at variance with results established by Professor Clausius, as has been asserted by Professor Osborne Reynolds in 'Nature,' vol. xvii. p. 122.

Part I.—*Treatment of the Problem by General Mechanical Considerations.*

1. If a drop of water or other volatile liquid is allowed to fall into a smooth and sufficiently hot metal dish, it continues a liquid drop instead of spreading out or flashing off into vapour, and it exhibits an appearance of great mobility. The drop is then in what has been called the "spheroidal state." Now, when a drop of liquid is so situated a chink may be observed between it and the hot surface beneath; so that the drop does not rest directly upon the metal, but is in reality floating upon a layer of vapour. We further learn

* It is known that gases feebly conduct heat by diffusion, and that the amount of heat which passes in this way between a heater and cooler is independent of the density of the intervening gas, provided that the density of the gas does not fall below a certain limit. The question that presented itself was as to what happens below that limit.

from these observations that after the brief interval of adjustment is over the layer of vapour presses upwards and downwards more than it presses sideways; for the pressure sideways must *equal* the pressure of the atmosphere so soon as the adjustment is over, otherwise air would still be entering or leaving the chink, whereas the pressure upwards must *exceed* the pressure of the atmosphere by an amount able to support the drop. It is my object to explain how this difference of pressures, this Crookes's pressure as it has been called, comes into existence.

2. The thermal conditions of the problem are easily traced, but need not detain us here. It is enough to state that they show the metal dish and drop to be at different temperatures, so that they are a heater and cooler on either side of the layer of vapour. Experiment further shows that the heater and cooler may be either one a liquid and one a solid, as in the case already considered, or both liquids, or both solid, and that the intervening layer may be either vapour or permanent gas. This last important fact has been established by Mr. Richard Moss in an admirable series of experiments lately made by him to test the theory of the present communication (see 'Scientific Proceedings of the Royal Dublin Society,' vol. i. p. 89). It is also found to be immaterial whether the heater or cooler is uppermost, or whether they face one another sideways.

Other facts of importance have been elicited by the experiments at low tensions, of which the most significant are:—that when the heater and cooler are maintained at given temperatures the Crookes's stress between them may be increased either by bringing the heater and cooler closer together, or by attenuating the gas until a certain point is reached which varies from one gas to another; and that when that point is passed, the force decreases and apparently without limit.

3. We may express these facts in a very convenient form for our present purpose if the heater and cooler are extensive flat parallel surfaces at fixed temperatures. Conceive two exactly similar patches on the heater and cooler directly opposite to each other, and each occupying a unit of surface, and consider that portion of space which lies between these. Then the observations show that there is one definite quantity of the gas to be left in the volume so marked out, if we wish to produce the strongest Crookes's stress. And, further, by comparing Mr. Crookes's experiments on the mechanical action with those of De la Provostaye and Desains on the flow of heat, we learn another important fact, viz. that the maximum stress occurs when the quantity of gas is too little to admit of

the passage of heat under the laws of the conduction of heat in gases. Now these facts also follow as consequences from the theory advanced by the author, and therefore become confirmations of it.

4. This theory seeks to account for Crookes's force by showing that a layer of gas placed between a heater and cooler is in a polarized condition of such a kind that the stresses within the gas are different in different directions. Gas is polarized whenever the molecules within a spherical element of volume are moving towards different quarters with numbers or velocities that are not distributed alike in all directions, the velocities being measured from the centre of mass of these molecules. This definition excludes the case of mere wind, which is to be regarded as unpolarized gas travelling forward in a certain direction; but it includes the case of gas across which heat is making its way, which is the case with which we have here to deal.

5. Let us recur to the simple instance of a heater and cooler with extensive flat parallel surfaces maintained at constant temperatures, and with gas between them freed from the action of gravity, and which has had time to adjust itself to its position. Gas so circumstanced will become stationary in the ordinary sense of the word; *i. e.*, though in active molecular motion, it will have no currents like convection-currents or wind passing through it*. We have now to show that the stress across such a layer will be greater than the stress sideways.

6. Imagine a unit of suface marked out on either heater or cooler, and let perpendiculars to the surface be raised from the boundary of this enclosure. These will trace out a straight tube extending between the heater and cooler, and closed at the ends by equal patches of the heater and cooler. These we may call the pistons of the tube. The molecules which strike the pistons are returned by them, and with altered velocity whenever the pistons are at different temperatures; but molecules pass without hindrance through the sides of the tube. Now it is evident that, if the molecules passing through an element of the side of the tube are considered, those passing out in a unit of time will be an exact counterpart of those passing in, in such a sense that the state of the gas would not be disturbed by making the sides of the tube im-

* There will be currents close to the boundary of the heater and cooler; but these are secondary phenomena caused by, and in no degree the cause of, Crookes's stress. They will not be appreciable within the layer at any considerable distance from the edge; and they may be avoided by giving to the opposed surfaces of the heater and cooler the form of concentric spheres.

pervious to molecules, provided that they were made at the same time *perfect reflectors* of molecules. By a reflector of molecules is to be understood a surface endowed with the property of throwing off any molecules that impinge upon it with unabated speed, and at an angle of reflection which lies in the same plane as the angle of incidence, and is equal to it. The reflected molecules will affect the state of the gas within the tube exactly in the same way as the molecules passing in from outside had done before. *We have now a portion of gas completely shut up* inside a tube with sides that are perfect reflectors of molecules, and closed at the ends by pistons that are patches of the heater and cooler, and which therefore scatter such molecules as reach them; and we know that the behaviour of this gas will be the same as that of the corresponding portion of the Crookes's layer. We may call such a tube a unit reflecting tube.

7. Let the pistons of such a tube be kept at the temperatures T_1 and T_2, and let gas be introduced into it. After a brief period of adjustment the gas will become stationary; *i. e.*, if a plane forming a cross section of the tube be considered, the molecular motions are such that the same number of molecules pass forwards as backwards through this plane per second. But they will pass it with unequal average velocities, because the *vis viva* of those crossing it towards the cooler must exceed the *vis viva* of those crossing it towards the heater by an amount bearing a known ratio to the quantity of heat advancing. Hence the gas is polarized, the molecular motions being swifter when they are directed forward or towards the cooler, and slower when directed backwards.

8. Suppose that we begin with dense gas and gradually exhaust, and let us consider the succession of events that will arise as the exhaustion proceeds, *i. e.* when n, the number of molecules in the unit tube, is progressively diminished. It is known that the flow of heat cannot conform to the laws of "conduction" unless the number of molecules exceeds a certain limit which we may call N,—N depending upon the description of gas that is present, and upon the temperatures T_1 and T_2 of the pistons which close the unit tube. We must therefore divide the exhaustion into two periods, one lasting while the number of molecules in the tube exceeds N, and the other during the rest of the exhaustion. Throughout the first period the flow of heat follows the known laws of conduction, and therefore remains constant. Hence, during this part of the exhaustion the polarization of the gas (which may be measured by $\frac{\delta v}{v}$, v being the average velocity at any point

of the layer, and δv the average difference of the velocities forwards and backwards at that station) is so rapidly on the increase as quite to compensate in $K\rho v^2\delta v$ (the expression for the flow of heat. ρ being the density at the station, and K a constant) for the diminishing density. During the second period, *i. e.* when the molecules have become fewer than N, the polarization is still on the increase, but not so rapidly as before, and at the same time the flow of heat decreases to zero; for while ρ tends to zero as the exhaustion proceeds, the polarization does not tend to infinity, but to a limit, viz. $\frac{v_1-v_2}{\sqrt{v_1v_2}}$, where v_1 and v_2 are the velocities corresponding to T_1 and T_2, the temperatures of the pistons. Now, when gas is polarized with this kind of polarization within a tube the sides of which reflect the molecules, we can find limits between which its thermal and mechanical properties must lie.

9. Before proceeding to determine these limits, it will be well to guard ourselves against making mistakes by passing under review the orders of the several magnitudes with which we are dealing in this inquiry. No *accurate* measures appear yet to have been made of the thickness of the chinks of air or vapour on which spheroidal drops rest. But from approximate measures, some of which were made by Mr. Fitzgerald and some by myself, I think it may be inferred that this thickness is somewhere about the thickness of a sheet of paper (*i. e.* about a fourth-metre or the tenth of a millimetre) when a spheroidal drop of the density of water, at a temperature of 10° C. and 5 or 6 millims. in diameter, floats over a surface of liquid which is about 10° warmer. We further know that in this case the Crookes's pressure, as it supports the weight of this drop, must be about the two-thousandth part of an atmosphere. These determinations are very rude; but they at all events tell us what kind of magnitude we are dealing with, and therefore suffice for our present purpose. They show that we shall not be *far* wrong in assuming definitively that the phenomenon presented by experiment which we have to explain is, that the stress across a stratum of air will be $\frac{1}{2000}$ part of the stress at right angles to that direction, if this stratum occupies the space between a heater and cooler at temperatures of 10° and 20° C., if, moreover, this interval is a fourth-metre (a metre divided by 10^4), and if the atmosphere has free access to the stratum of air at its edges. Let us now imagine a reflecting tube, such as is described above, to be placed across this stratum. It will therefore be a fourth-metre long; and we may assign to it

any width we please. Let us take a width equal to the diameter of the smallest object that can be seen with a microscope, which is about 2·5 seventh-metres, or the 100,000th part of an inch. We have now to compare the dimensions of this tube with the number and motions of the molecules included within it. The number of molecules in a cubic millimetre of atmospheric air is about a unit-eighteen (10^{18}). (See Phil. Mag. August 1868.) Whence the average interval between them is about a ninth-metre. This is the 100,000th part of the length of our tiny tube and the 250th part of its breadth. Hence the tube will contain a vast number of molecules, some such number as five thousand millions. Again, the average striking-distance (*i. e.* the average length of path between the encounters) of the molecules is about the 1500th part of the length of the tube, or the fourth part of its breadth. There is, therefore, abundant room within the tube, small as it is, for a vast number of molecules and for much jostling amongst them. The temperatures with which we are dealing are such that the average velocity with which the molecules of air dash about may be taken as 500 metres per second; and the molecules meet with so many encounters, that the direction of the path of each is changed somewhere about a unit-ten of times (10,000,000,000) every second. To complete the picture, we must remember that each molecule is in a state of vigorous internal motion as well as travelling about among its fellows, and that when an encounter takes place, the energy which passes from one molecule to another is employed in changing both those kinds of motion, and possibly (but not probably) another part becomes potential energy, *i. e.* energy expended in altering the configuration of the parts of the molecule, or the position of its parts with reference to the æther. The motions which go on within the molecules are what give rise to the linear spectra of gases, and are therefore those motions of the gas that act on the æther, and are in turn partly controlled by it*. They are recurring motions which, at least in some cases, are resolvable either

* May we not look, with some prospect of success, to the control which is exercised by the æther on the internal motions of the molecule for the explanation of the number of "degrees of freedom" of a molecule, which (on the supposition that there is no potential energy) is in most gases 5 (see Watson's 'Kinetic Theory of Gases,' p. 39). The number 5 appears to indicate that the motions within the molecules are trammelled, as here suggested. This view is, moreover, supported by the fact that light is emitted by the gas, which could not be the case unless vast numbers of molecules moved in unison with one another; and the most probable account of this appears to be that they are all trammelled in the same way by their common relation to the æther.

into harmonics like the vibrations of a string, or else into quasi-harmonics not to be distinguished from harmonics by observation (see Donkin's 'Acoustics,' § 194), like the transverse vibrations of an elastic rod—probably the former. On the more probable supposition that they are true harmonics, the periodic times have been determined with great precision in some cases, notably in the cases of a motion within the molecules of hydrogen, which gives rise to three of its spectral lines, and a motion within the molecules of chlorochromic anhydride, which gives rise to 105 of its spectral lines. In hydrogen the motion is repeated as often as 2,280,000,000,000 times each second in every molecule, and in the vapour of chlorochromic anhydride rather more than 800,000,000,000 times*. Such are the periodic times on the supposition that the motions are resolvable into true harmonics; and whether the fact be that the components of the motions are harmonics or quasi-harmonics, their periodic times are at all events quantities of this order. The general presumption, therefore, is that the periodic times within the molecules of other gases are also quantities of this order. But it is not necessary for our present purpose to establish this. The only circumstance relating to these inner motions with which we are here directly concerned is that the energy which is transferred from molecule to molecule is employed partly in altering the velocities with which the molecules travel about, and partly in altering these internal motions and (perhaps) collocations, and that the proportion of the energy which is employed in the former way bears on the average a numerical ratio to the whole energy transferred which can be determined experimentally (see Maxwell's 'Theory of Heat,' p. 299) and is denoted in the sequel by $\frac{1}{\beta}$.

10. We now proceed to determine limits between which the thermal and mechanical properties of the gas must lie. For this purpose let us imagine a tube of the kind described

* The periodic times deduced from the observations are respectively $\frac{\tau}{76{\cdot}18}$ and $\frac{\tau}{2{\cdot}70}$, τ being the time that light takes to advance 1 millim. *in vacuo*. (See Phil. Mag. April 1871, p. 295, and July 1871, p. 45. In the former paper read 0·013127714 for 0·13127714.) The first of these determinations was made by the present author, and the second by the present author in conjunction with Professor Emerson Reynolds, of Dublin; but, before either of these determinations were made, Professor Clifton, of Oxford, had mentioned at the Exeter Meeting of the British Association in 1869 that he had found two of the hydrogen lines (probably C and h) to be related harmonically. I am not aware that any record of this important observation has been published.

above, with perfectly reflecting sides. Such a tube exerts no friction on gas flowing along it, nor does it occasion any loss of energy. Let it contain a large number of gaseous molecules between pistons at temperatures T_1 and T_2. And let us further suppose that the molecules of the gas, as they leave either piston, acquire the property of not interfering with or being obstructed by the molecules that have last left the other. This imaginary state of the gas would re sult in two streams constantly travelling in opposite directions along the tube. Let us follow one of these streams. It starts from its piston with a mean of the squares of the velocities of its molecules v_1^2 determined by the temperature of the piston, and in numbers per unit of time represented by $\rho' u'$, ρ' being the density of the stream and u' the average of the normal components of the velocities at starting. Then, however the velocities and directions may have been distributed at starting, *the jostling of the molecules of this stream among one another will reduce the stream as it advances to the condition of unpolarized gas travelling along the tube with the velocity* u'. The molecules are henceforward moving with velocities among themselves which, measured from their advancing centre of mass, have an average square of the velocities w'^2 which is given by the equation

$$\beta v_1^2 = u'^2 + \beta w'^2, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

β being the known numerical coefficient representing the ratio of the total energy of the gas to its "energy of agitation." This equation is only the symbolical expression of the fact that no energy has entered or left the gas. The stream moving in the opposite direction furnishes the similar equation

$$\beta v_2^2 = u''^2 + \beta w''^2. \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

And since the numbers of molecules reaching and receding from each piston are equal, we have the further equation

$$\rho' u' = \rho'' u''. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

We have also

$$\rho = \rho' + \rho''. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (4)$$

Of the quantities which enter into these equations, ρ, the density of the gas, is known, and v_1, v_2, u', and u'' are known functions of T_1 and T_2, the temperatures of the pistons. Hence these equations enable us to determine the remaining quantities ρ', ρ'', w', and w''.

Now, under the conditions that have been laid down, it is

manifest that the stress * of the gas sideways would be

$$P_y = \tfrac{1}{3}\rho' w'^2 + \tfrac{1}{3}\rho'' w''^2, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (5)$$

while the stress along the tube would be

$$P_x = \tfrac{1}{3}\rho' w'^2 + \tfrac{1}{3}\rho'' w''^2 + \rho' u'^2 + \rho'' u''^2, \quad . \quad . \quad (6)$$

which accordingly exceeds the transverse stress by

$$\kappa = \rho' u'^2 + \rho'' u''^2. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (7)$$

This, therefore, would be the Crookes's stress in the case supposed. It is a very large quantity, since u' and u'' would be large if the streams could penetrate one another without obstruction. The flow of heat, which we will designate by the symbol G, would also be very large in the case supposed. An expression for it can be easily found, but is not required for our present purpose.

11. The other limit is one that really occurs. It arises when the molecules coming up to either piston and those retiring from it form complementary parts, such that their coexistence in the same space constitutes stationary unpolarized gas. This happens only when the two pistons are at the same temperature. In this case it is manifest that no heat is conveyed by the gas, and that the gas exerts the same pressure in all directions. In symbolical language,

$$G = 0,$$
$$\kappa = 0,$$

G being, as before, the symbol for the flow of heat, and κ for the Crookes's stress. This case may be described as one in which the streams described in the last section experience such effectual opposition from each other that the speed with which they advance is zero. For it is evident that the gas at any station within the tube may, without any change of its properties, be described as consisting of two equal portions of stationary unpolarized gas coexisting in the same space.

12. In all other cases the pistons that close the ends of the unit tube are at different temperatures, and the gas between any two cross sections of the tube is polarized. Let us consider a slice between two such sections, which are sufficiently close to entitle us to regard the included gas as being throughout in nearly the same state. The actual condition of the gas

* The term "stress" is here applied to the *pressure* within the gas in any direction, viewed in conjunction with the equal pressure in the opposite direction. It is what Clausius has called "the positive momentum," meaning thereby the sum of the components of the momenta of the molecules resolved in a given direction, and all estimated as positive, whether of molecules that move forward or backward.

within the slice may evidently be conceived of as arising from the coexistence of two streams travelling in opposite directions along the tube, and each consisting of gas which is less polarized (*i. e.* which deviates less from the condition of ordinary gas) than the gas that results from their coexistence. Each stream is exposed within the slice to the mutual jostling of its own molecules; and it is also attacked by molecules of the other stream. The mutual jostling of its own molecules tends, as explained in section 10, both to maintain the onward velocity of the stream and to reduce the gas of which the stream consists still more towards the condition of unpolarized gas. These encounters then, taken by themselves, tend to bring about the state of the gas described in section 10. But the interference of the two streams with one another counteracts this. This interference modifies the effect of the encounters within the streams, but it is incompetent to annul it; for the two streams do not by their mere coexistence constitute stationary unpolarized gas, and hence they would need time before they could by their action upon one another reduce the gas to this condition. It is, however, plain that whatever action they exert is a step towards bringing about this condition; for the gas would become depolarized if the cross sections which bound the slice could be rendered impervious both to energy and molecules, so as to leave the two streams time to act fully on one another. In reality, however, sufficient time is not allowed to them, because the streams pass one another, and the struggle is continually renewed within the slice by fresh portions of the streams which come up *in the same state* as those that had been obliged to pass on. These fresh portions keep in the same state because a sufficient supply of swift molecules is without intermission being thrown back along the tube from one end by the heater, and a corresponding supply of slow molecules from the other end by the cooler.

13. The two streams, however, though not annulled, are different from what they would have been if they had been without influence upon one another. They do not consist of the same molecules from one instant to another; for there is such a perpetual shifting of molecules between them, owing to the vast number of encounters that take place, that no one molecule is likely to remain long in one stream. Again, after an encounter between molecules of the two streams, both of the colliding molecules will sometimes join the same stream; and it will most frequently happen that the stream so joined is the hotter and swifter stream. Hence the stream from the heater to the cooler receives an accession to the number of its

molecules as it travels forward, while the reverse effect is produced upon the stream making its way in the opposite direction. On both accounts there will be gradients of density and temperature along the tube between the heater and cooler. Again, every encounter between molecules of the two streams diminishes the momentum of one or both streams; but, as we have seen, the effect so produced does not go the length of reducing the streams to rest.

14. Hence we must bear in mind the gradients of temperature and density along the two streams, and the continual fluctuation of the molecules that are to be referred to them, if we want to regard the condition of the gas throughout the whole length of the tube as arising out of the coexistence of two streams of gas less polarized than itself. But with these precautions the hypothesis may be made; and accordingly the condition of the gas at every cross section of the tube *is intermediate between a structure represented by the coexistence of the two streams of unpolarized gas travelling simultaneously in opposite directions*, which the encounters within each stream tend to develop, *and the condition of stationary unpolarized gas*, towards which the mutual interference of the two streams modifies the structure. Hence there is some polarization stress and some flow of heat all along the tube, though of less amount than in the case considered in section 10. We may still employ equation (7) as the expression for the polarization stress, if we use for ρ' and ρ'' the densities of the streams at some particular cross section of the tube, and if u' and u'' are modified into what they become as the interference of the two streams with one another is increased. It is not necessary to ascertain what this modification will be: it is enough for our purpose to know that u' and u'' will be some functions of $V'-V''$ (where V'^3 and V''^3 are the averages of the cubes of the velocities of the molecules that pass forwards and backwards respectively through the cross section), and that they will be proportional to this quantity when all three are small.

15. We may base upon this circumstance an investigation into the laws of the phenomenon when the difference between the temperatures of the heater and cooler is small compared with their absolute temperatures. This case is of importance because it is that which most frequently occurs, and is the only one in reference to which accurate experiments have been made. In this case ρ' and ρ'' will each be nearly $\frac{1}{2}\rho$, using ρ for the density of the gas at the position in the tube which we are considering; and $V'-V''$, being small, may be appropriately represented by δV. Then, remembering that u' and u'' are proportional to δV, we obtain from equation (7)

the following expression for the polarization stress,

$$\kappa \propto \rho(\delta V)^2, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (8)$$

where the symbol $\propto$ means *approximately varies as.* Moreover it can be shown* from various considerations that the flow of heat

$$G \propto \rho V^2 \delta V,$$

or, by a simple transformation,

$$G \propto \rho T . \delta V, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (9)$$

T being the temperature measured from absolute zero. Hence from the approximate equations (8) and (9) we obtain the equation

$$\kappa \propto \frac{G^2}{\rho T^2}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (A)$$

which contains only quantities of which we already know enough to make use of them. Equation (A) may be thrown into a still more convenient form by writing P for the tension of the surrounding atmosphere of gas, which is nearly the same as the stress which the gas at the station we are consi-

* One of the ways in which this may be proved is the following:— Clausius has shown (Phil. Mag. xxiii. p. 514) that

$$G = \tfrac{1}{4}\beta\rho \int_{-1}^{+1} I\overline{V^3}\mu d\mu,$$

whence

$$G = \tfrac{1}{8}\beta\rho(I'V'^3 - I''V''^3),$$

where I′ and V'^3 are the average values of I and $\overline{V^3}$ under the integral for positive values of μ (*i. e.* for molecules traversing the section of the tube towards the cooler), and I″ and V''^3 are the corresponding averages for negative values of μ, *i. e.* for molecules traversing the section of the tube in the opposite direction.

Now it is evident that these quantities are capable of expansion in the following form:—

$$I' = 1 + A_1\frac{\delta V}{V} + A_2\frac{(\delta V)^2}{V^2} + \ldots,$$

$$I'' = 1 + B_1\frac{\delta V}{V} + \ldots,$$

$$V'^3 = V^3\left(1 + C_1\frac{\delta V}{V} + \ldots\right),$$

$$V''^3 = V^3\left(1 + D_1\frac{\delta V}{V} + \ldots\right),$$

in which V^3 is the average of the values of $\overline{V^3}$ for all directions. Whence

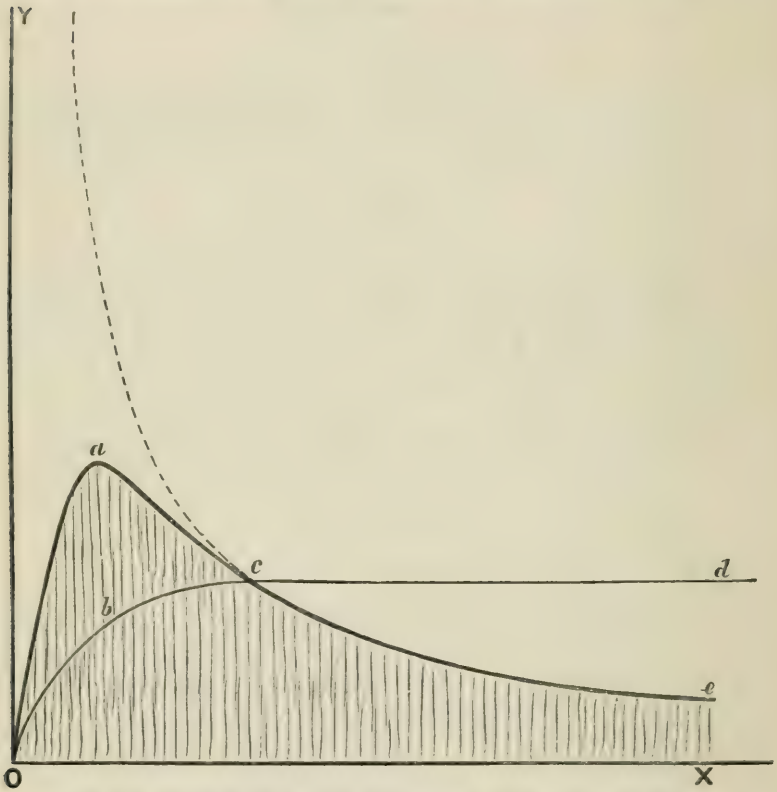
$$G = \tfrac{1}{8}\beta\rho(A_1 - B_1 + C_1 - D_1)V^2\delta V$$

+ terms containing higher powers of δV.

dering would exert if depolarized. P will therefore vary nearly as ρT, whence

$$\kappa \propto \frac{G^2}{PT}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (B)$$

16. As an example of the application of these approximate



formulæ, let us plot down on a diagram the value of κ, the polarization stress, for various tensions of gas between a heater and cooler at constant temperatures and at a fixed distance asunder.

Let the abscissas of the figure represent the tensions of the gas. Then the curve O *b c d*, the ordinates of which represent the flow of heat, is known. The part representing conduction is the parallel line *c d*; and O *b c* represents the outflow of heat by that modified conduction which may be called penetration, which occurs when the exhaustion has proceeded so far that the number of molecules in a unit tube is less than N (see

above, section 8). The curve O *b c d* is therefore known; and if by equation (B) we plot down from it the values of κ (the polarization stress), we find them approximately represented by the ordinates of a curve of the form O *a c e*, the portion to the right of *c* being coincident with an equilateral hyperbola, while to the left of *c* the ordinates fall short of the hyperbola, rising to a maximum and then falling off to zero. The position of this maximum cannot be obtained with certainty, because equation (B) is less to be depended on at *very* low tensions. Bearing this in mind, the accordance of the theoretic values with those determined experimentally by Mr. Crookes and Mr. Moss is satisfactory.

17. From equation (B) we may obtain another useful formula which expresses the approximate law according to which polarization stress depends upon the interval between heater and cooler, whenever this interval exceeds the limit determined by the condition that there shall be a sufficient number of molecules in the unit tube to allow heat to pass by conduction.

In this case we know the equation of the gradient of temperature (see Clausius' equation (54), Phil. Mag. vol. xxiii. p. 527), and that it is approximately represented by a straight line when, as we have assumed, $\frac{\Delta T}{T}$ is small, using ΔT for the difference between the temperatures of the heater and cooler. Hence, and from Clausius' equation (56), it appears that

$$\frac{G^2}{T} \propto \frac{(\Delta T)^2}{X^2},$$

using X for the distance between the heater and cooler. Introducing this value into equation (B), we find

$$\kappa \propto \frac{(\Delta T)^2}{PX^2}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (C)$$

a result which agrees satisfactorily with Mr. Moss's experiments.

18. If we use X_0 for that interval between heater and cooler which would make the number of molecules in the unit tube equal to N, and if we use κ_0 for the corresponding value of the Crookes's stress, then equation (C), and the obvious equation $X_0 \propto \frac{1}{P}$, furnish us with the following:—

$$\kappa_0 \propto P \,.\, (\Delta T)^2. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (10)$$

Now equation (C) enables us to plot down a *part* of the curve representing the relation between κ and X when ΔT and P are kept constant; and although equation (C) cannot be relied upon when X is less than X_0, it is nevertheless evident that

the shape of the remainder of the curve must be one which is independent of the particular value of P which we have used. Hence, if κ is the maximum value of κ in that curve, it follows that $\bar{\kappa}$ and κ_0 must remain proportional to one another when P is changed. Hence equation (10) furnishes

$$\kappa \propto P.(\Delta T)^2. \quad . \quad . \quad . \quad . \quad . \quad . \quad (D)$$

We learn from this inquiry that the maximum polarization stress which can be elicited between a given heater and cooler by varying the distance between them will, if the tension of the gas is altered, change in the same ratio as that tension, and that it will occur at intervals between heater and cooler which vary inversely as that tension. This fully accounts for the powerful Crookes's force which presents itself in experiments at ordinary atmospheric tensions as compared with the feeble force exhibited in radiometers. It accounts also for the very short interval at which the heater and cooler must be placed when the gas is dense.

Part II. *Investigation of a Complete Expression for the Stress.*

19. As it has been asserted ('Nature,' vol. xvii. p. 122) that the views of the present writer are at variance with the results established by previous investigators, I will proceed to show that the theory of unequal stresses which I have put forward is, on the contrary, the necessary sequel of them. I will show this by continuing the method of investigation commenced by Professor Clausius in his memoir on "the Conduction of Heat by Gases," in the way which was pointed out by Mr. George F. Fitzgerald in 'Nature,' vol. xvii. p. 200. This inquiry will have the further advantage of furnishing a complete expression for Crookes's stress.

Clausius (Phil. Mag. vol. xxiii. p. 514) has given the following expression for the stress across a layer of gas conducting heat, in the direction *normal* to a heater and cooler, the opposed surfaces of which are parallel and extensive,

$$P_x = \tfrac{1}{3}\rho v_y^2 + X_1\epsilon^2,$$

ϵ being a small quantity of the same order as the striking-distance of the molecules, and X_1 being a coefficient of which Clausius did not compute the value, as the scope of his investigation only required him to go as far as the first order of small quantities. Now Mr. Fitzgerald, in his letter to 'Nature,' and more fully in conversation with the writer, pointed out that if an expression for P_y, the stress *parallel* to the surfaces of the heater and cooler, were calculated by a method similar to Clausius', the coefficient of ϵ^2 in this expression *could* not

be the same as X_1, and that hence there must be a difference between the two stresses—in other words, a polarization stress.

20. Clausius (*loc. cit.*) gives the following general expression for the normal stress—

$$P_x = \tfrac{1}{2}\rho \int_{-1}^{+1} I\overline{V^2}\mu^2 d\mu, \quad . \quad . \quad . \quad . \quad . \quad (11)$$

where I is the coefficient expressing the proportion of molecules travelling in the directions which make an angle with the normal or axis of x of which the cosine is μ, and where $\overline{V^2}$ is the mean of the squares of their velocities.

Now if, employing a process exactly similar to that pursued by Clausius on pp. 512 and 513 of his memoir, we use N for the number of molecules in a unit of volume, then will Ndr be the number of molecules within a slice of unit area and thickness dr, which we may suppose to be placed perpendicular to the vector r. Then

$$\frac{1}{4\pi} NI dr d\sigma$$

will be the number of molecules moving within the slice in directions which lie within an element of solid angle $d\sigma$, which we will suppose makes the angle ψ with the vector r; so that the time they take to cross the slice will be

$$\frac{dr \,.\, \sec\psi}{V},$$

V being their velocity. Hence the number traversing the slice in the specified direction within a unit of time is

$$\frac{1}{4\pi} \,.\, NIV \cos\psi \,.\, d\sigma.$$

Multiplying this by mV $\cos\psi$ we get the resolved part of their momenta along r. The sum of all such components of the momenta, all estimated as positive, is P_r, the stress in the direction of r. Whence, and writing ρ for mN, we find

$$P_r = \frac{\rho}{4\pi} \iint I\overline{V^2} \cos^2\psi d\sigma,$$

the integration being extended over the unit sphere.

Hence the stresses in the directions of three rectangular axes are

$$P_x = \frac{\rho}{4\pi} \iint I\overline{V^2} \cos^2\alpha d\sigma,$$

$$P_y = \frac{\rho}{4\pi} \iint I\overline{V^2} \cos^2\beta d\sigma,$$

$$P_z = \frac{\rho}{4\pi} \iint I\overline{V^2} \cos^2\gamma d\sigma,$$

α, β, γ being the director angles of the element of solid angle $d\sigma$. Introducing polar coordinates, we have

$$d\sigma = \sin\theta\, d\theta\, d\phi,$$
$$\cos\alpha = \cos\theta,$$
$$\cos\beta = \sin\theta\cos\phi,$$
$$\cos\gamma = \sin\theta\sin\phi,$$

by which the expressions for the stresses become

$$\left.\begin{aligned} P_x &= \frac{\rho}{4\pi}\int_0^{2\pi}\int_0^{\pi} I\overline{V^2}\cos^2\theta\sin\theta\, d\theta\, d\phi, \\ P_y &= \frac{\rho}{4\pi}\int_0^{2\pi}\int_0^{\pi} I\overline{V^2}\sin^3\theta\cos^2\phi\, d\theta\, d\phi, \\ P_z &= \frac{\rho}{4\pi}\int_0^{2\pi}\int_0^{\pi} I\overline{V^2}\sin^3\theta\sin^2\phi\, d\theta\, d\phi. \end{aligned}\right\} \quad . \quad . \quad \text{(E)}$$

These are the most general expressions for the stresses in three rectangular directions within gas polarized in any way; and they will be the only stresses between portions of the gas separated by planes parallel to the planes yz, zx, xy, if the axes are so chosen that there are no moments round them arising from the molecular encounters*.

21. This condition is easily secured in the case which we are investigating, viz. when heat is making its way between a heater and cooler that are parallel to one another, and of large extent compared with the interval between them, since the polarization of the intervening gas will evidently be disposed symmetrically round the direction in which the heat is travelling. Hence, taking this direction as our axis of x, there can be no moments round this axis, or round any axis at right angles to it. The stresses (E), therefore, are the only ones to be taken into account. Moreover we can integrate equations (E) at once by ϕ, since $I\overline{V^2}$ is, in this simple case, a function

* Equations (E) cannot be integrated unless $I\overline{V^2}$ is given as a function of θ and ϕ, *i. e.* unless the law of polarization in the gas is known. But they show that in general *the stresses in different directions are unequal,* which is here what is chiefly insisted on.

When the gas is unpolarized, I becomes equal to unity, and $\overline{V^2}$ is independent of the direction, and may therefore be put outside the integrals. In this case all three equations concur in giving the well-known expression for the stress in unpolarized gas, viz. $\frac{1}{3}\rho\overline{V^2}$.

of θ only. Doing this, and writing μ for $\cos\theta$, we find

$$\left.\begin{aligned} P_x &= \frac{\rho}{2}\cdot\int_{-1}^{+1} I\overline{V}^2\mu^2 d\mu, \\ P_y = P_z &= \frac{\rho}{4}\int_{-1}^{+1} I\overline{V}^2(1-\mu^2)d\mu, \end{aligned}\right\} \quad . \quad . \quad (F)$$

whence, since κ, the polarization stress, $= P_x - P_y$, we have finally

$$\kappa = \frac{\rho}{4}\int_{-1}^{+1} I\overline{V}^2(3\mu^2-1)d\mu. \quad . \quad . \quad . \quad . \quad (G)$$

This, then, is the complete mathematical expression for Crookes's stress. It could be integrated if we knew the law of the polarization of the gas; for then $I\overline{V}^2$ would be a known function of μ.

22. Clausius, in investigating the diffusion of heat across the layer of gas, makes the assumption (Phil. Mag. vol. xxiii. pp. 425 and 524) that the numbers and velocities of the molecules "emitted" by a thin stratum of the gas (*i. e.* that have passed out of the stratum after having encountered other molecules within it) may be adequately represented "by assuming at first motions taking place equally in all directions, and then supposing a small additional component velocity in the direction of positive x to be imparted to all the molecules." In other words, it is assumed that the motions of these molecules may be represented by radii vectores from a slightly excentric origin to points equally distributed over the surface of a *sphere.* It will be instructive to trace the consequences of this hypothesis, *both because of what it will do and what it will not do.*

Upon this hypothesis Clausius finds the following convergent series for $\overline{V}^2$ and I (*loc. cit.* pp. 434 and 516):—

$$\overline{V}^2 = u^2 + 2uq\mu\epsilon + (2ur + q_1^2)\mu^2\epsilon^2 + \ldots,$$

$$I = (1 - \tfrac{1}{3}r'\epsilon^2 + \ldots) - \frac{q}{u}\cdot\mu\epsilon + r'\mu^2\epsilon^4 + \ldots,$$

where $\frac{1}{5}q\epsilon$ (*loc. cit.* p. 525) is the small component velocity spoken of above, u is the mean velocity of molecules moving in the plane yz, and the other letters have the meanings assigned to them by Clausius. Multiplying these together, going to the second order of small quantities, and arranging by powers of μ, we find

$$I\overline{V}^2 = u^2(1 - \tfrac{1}{3}r'\epsilon^2) + A_1\mu\epsilon + A_2\mu^2\epsilon^2, \quad . \quad . \quad (12)$$

where

$$A_2 = -2q^2 + 2ur + q_1^2 + u^2r'. \quad . \quad . \quad . \quad . \quad . \quad (13)$$

Introducing the expression (12) into equations (F) and (G) we find

$$\left.\begin{aligned} P_x &= \tfrac{1}{3}\rho u^2(1-\tfrac{1}{3}r'\epsilon^2)+\tfrac{1}{5}\rho A_2\epsilon^2+\dots, \\ P_y &= P_z = \tfrac{1}{3}\rho u^2(1-\tfrac{1}{3}r'\epsilon^2)+\tfrac{1}{15}\rho A_2\epsilon^2+\dots, \\ \kappa &= \tfrac{2}{15}\rho A_2\epsilon^2+\dots. \end{aligned}\right\} \quad . \quad . \quad (14)$$

In these A_2 stands for the expression (13); and introducing the following values, which are given by Clausius as correct to the second order of small quantities (*loc. cit.* p. 526, footnote),

$$q_1^2 = \frac{41}{25}q^2,$$

$$ur = -\frac{31}{50}q^2,$$

$$u^2r' = \frac{266}{17{\cdot}25}q^2,$$

we find

$$A_2 = 13{\cdot}8q^2.$$

From this and (14),

$$\kappa = 1{\cdot}8 \times \rho q^2\epsilon^2 + \text{terms of the fourth and higher orders.} \quad (15)$$

But by Clausius' theory (*loc. cit.* p. 516),

$$G = \tfrac{1}{3}\beta\rho u^2 q\epsilon + \text{terms of the third and higher orders.} \quad . \quad (16)$$

Whence, approximately, omitting the fourth and higher orders of small quantities, and writing v for u, since they are nearly equal, and then putting P for its equivalent $\frac{1}{3}\rho v^2$,

$$\kappa = 1{\cdot}8\,\frac{\rho G^2}{\beta^2 P^2}. \quad . \quad . \quad . \quad . \quad . \quad . \quad (17)$$

Now, by Boyle and Charles's law,

$$\frac{P}{P_0} = \frac{\rho T}{\rho_0 T_0},$$

where P_0, ρ_0, and T_0 have reference to standard temperature and pressure. Whence, finally,

$$\kappa = \left[(1{\cdot}8)\,\frac{\rho_0 T_0}{\beta^2 P_0}\right]\cdot\frac{G^2}{PT}, \quad . \quad . \quad . \quad . \quad . \quad (18)$$

an equation which assigns the same law as we obtained above in equation (B) by the wholly different method of direct mechanical considerations.

23. Equation (18) *appears* to give also the amount of the polarization stress. But this is illusory. The hypothesis upon which it rests is adequate as regards the conduction of heat, but is insufficient for a quantitative investigation of the stress, as I will now proceed to show.

The general formulæ for the conduction of heat and for the polarization stress are the following—

$$G = \tfrac{1}{4}\beta\rho\int_{-1}^{+1} I\overline{V}^3 . \mu d\mu,$$

$$\kappa = \tfrac{1}{4}\rho\int_{-1}^{+1} I\overline{V}^2 . (3\mu^2-1)d\mu$$

(see Clausius' memoir, p. 514, and equation (G) above). Now μ and $3\mu_2-1$, which occur as factors in these integrals, are the first and second terms of a series of spherical harmonics (Laplace's coefficients) of the simple kind that are functions of μ only, and which therefore represent the radii of solids *of revolution* from points on their axes. It is moreover obvious that we can expand $I\overline{V}^2$ and $I\overline{V}^3$ in series of spherical harmonics of the same simple type. Doing this,

$$I\overline{V}^3 = g_0 + g_1 + g_2 + \ldots,$$

$$I\overline{V}^2 = k_0 + k_1 + k_2 + \ldots,$$

the g's and k's representing spherical harmonics. Whence, and from the fundamental property of spherical harmonics,

$$G = \tfrac{1}{4}\beta\rho\int_{-1}^{+1} g_1\mu\delta\mu,$$

$$\kappa = \tfrac{1}{4}\rho\int_{-1}^{+1} k_2(3\mu^2-1)d\mu.$$

Hence g_1 is the only term of the first series that produces any conduction of heat, and k_2 is the only term of the second series that produces any polarization stress.

Let us suppose radii drawn from a point in all directions, of lengths proportional to the values of $I\overline{V}^2$ in those directions. We thus obtain a solid of revolution which may also be arrived at by plotting down radii equal to k_0, and successively correcting the solid so found by the addition of k_1, k_2, &c. to its radii. Now

$$k_0 = A,$$

$$k_1 = B . \mu,$$

$$k_2 = C . (3\mu^2-1),$$

&c. &c.

where A, B, C, &c. are independent of μ. In the case we are considering, B, C, &c. are small compared with A. From the foregoing values it follows that if k_0 is plotted down by itself, it will produce a sphere with its centre at the origin of radii.

Next, k_0+k_1 may be plotted down by shifting the centre of this sphere through the small distance B towards positive x, and by then very slightly distorting the form of the sphere. Again, to plot down k_0+k_2, we should elongate the sphere in the direction of the axis x by an amount equal to 4C, and narrow it equatorially by an amount equal to 2C, without shifting its centre. Finally, $k_0+k_1+k_2$ would be represented by radii drawn to the surface of this last solid, after it had been slightly distorted and removed through the distance B towards the cooler. Through all these mutations the mean value of the radii drawn from the origin remains unaltered*.

Comparing these figures with expansion (12), which is the value for $1\overline{V^2}$ furnished by Clausius' hypothesis, we find that the form and position of the solid which results from plotting it down are such that (owing to the term containing μ) there is that separation between the origin of radii and the centre of figure which gives a sufficient value to the function k_1, but that (the coefficient of μ^2 containing only very small quantities) the solid is not elongated in the way which would allow the function k_2 to attain any considerable value. That the function k_2 is not *wholly* absent is because of such causes as the slight distortion of figure before mentioned, which give rise to very small† terms of the form k_1, k_2, &c.

* Since, by the fundamental property of spherical harmonics,

$$\int_{-1}^{+1} k_1 d\mu = 0,$$

$$\int_{-1}^{+1} k_2 d\mu = 0,$$

&c.

† That k_2 is very small, if we adopt Clausius' hypothesis, may also be seen by comparing equation (18) with experiments on spheroidal drops. Observation shows that, at atmospheric temperatures and pressures, a spheroid of water some millimetres in diameter will be supported at a distance of about a fourth-metre (a metre divided by 10^4) from the heater, when the difference of temperatures is about 10° C. In G.C.S. (gramme, centimetre, second) systematic measures, the hyper-milligram ($\frac{10}{g}$ of the gravitation of a milligram, g being gravity measured in metres per second) per square centimetre is the unit of stress. Hence the Crookes's stress which supports this drop must amount to some hundreds of these units. This is the amount indicated by experiment.

Formula (18) assigns to it a very different value. Clausius estimates the flow of heat across air between a heater and cooler, each a square metre in surface and a metre asunder, and kept at temperatures which differ by 1° C., as amounting per second to $\frac{11}{4000000}$ of the quantity of heat which will warm a kilogram of water 1° C. About ten times this, or $\frac{11}{400000}$ of this calory per second, would be the flow of heat between

This almost total absence of the elongated form arises from Clausius' fundamental hypothesis, that the motions of the molecules emitted by a stratum may be represented by radii drawn to a *sphere* from an excentric point; whereas it appears from the discussion in the earlier part of the present memoir that the encounters that take place within each of the two streams into which the gas may be divided, give to the surface to which the radii are to be drawn an elongated form. This omission from Clausius' hypothesis does not sensibly affect the spherical harmonics of the *first* order, and accordingly his hypothesis is adequate as regards the flow of heat, which depends exclusively on one of these; but it renders the hypothesis an insufficient one as regards polarization stress, or any other phenomenon which depends on spherical harmonics of the *second* order.

LIV. *Researches on Unipolar Induction, Atmospheric Electricity, and the Aurora Borealis.* *By* E. Edlund, *Professor of Physics at the Swedish Royal Academy of Sciences.*

[Concluded from p. 371.]

As the magnetic properties of the earth cannot be fully accounted for by assuming the existence of a magnet in its interior, it is quite evident that the preceding consideration can only indicate the general course of the phenomena in question. We will now rapidly examine whether the results of that consideration are conformable to those given by observation.

Atmospheric electricity has long been the object of repeated investigations. These have been so numerous that to give an

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two square centimetres at the distance of a fourth-metre asunder and kept at temperatures that differ by 10° C. To turn this into kinetic measure we must multiply by 41000×1000000; so that G would amount to about 1144000 in G.C.S. kinetic measure (*i. e.* in hyper-fifth-grammetres per second). Again, we may take as rough approximations,

$$\rho_0 = \frac{1}{800},$$

$$T_0 = T.$$

$$\beta^2 = 2{\cdot}6,$$

$$P_0 = P_1 = 1000000.$$

Introducing these values into equation (18), we find approximately,

$$\kappa = 0{\cdot}001$$

of a hyper-milligram per square centimetre—an amount which, as it ought to be, is vastly smaller than that indicated by experiment.

account of them would require too much space ; and it is besides the less necessary as we already possess a statement, followed by a very well executed critical discussion, of those observations*. Some physicists have essayed to explain the results obtained by observation by assuming that the air itself is electropositive in its normal state ; while others have thought that the surface of the earth is electronegative, and that it produces by induction the electrical phenomena observed in the atmosphere. Opinions have been much divided upon the true cause of the electric state of the air or of that of the earth's surface. Some have assumed that evaporation from the sea, lakes, and the wet surface of the ground renders the atmosphere electropositive and the earth electronegative ; others, on the contrary, that the distribution of electricity has its efficient cause in vegetation, in the putrefaction of organic matters at the surface of the earth, in the friction of the air against the ground or the water, or in the condensation and rarefaction of the air, &c. More exact researches having shown that none of these opinions can be correct, the phenomenon has been attributed to a purely cosmic cause, having its seat in the empty space surrounding the earth, or in other celestial bodies, chiefly the sun. In a word, up to this hour no one has succeeded, either in consequence of observations, or by theoretic considerations, in assigning a valid and trustworthy cause for the electric phenomena of the terrestrial atmosphere. From the above-mentioned observations, however, it appears that the atmosphere, under normal conditions, can be regarded as electropositive in its lower strata (accessible to observation), that the amount of electricity increases with the height above the earth's surface, and that the amount is subject to a diurnal and an annual variation.

From the preceding exposition of the cause of unipolar induction it follows that the atmosphere must be electropositive, and the earth electronegative. The electric molecules at and beneath the earth's surface are directed by the force of terrestrial unipolar induction into the atmosphere, where they accumulate until they attain a certain density, dependent on the greater or less conductivity of the strata of air. It is only successively that, impelled by the same force, they arrive in the upper strata of the atmosphere, endowed with a high degree of conductivity, but certainly inferior to that of the metals. At the same time that the electric molecules rise above the surface of the earth, they are carried, in both hemispheres, from the lower into the higher latitudes, where the electric density

* *Om den s. k. luftelektriciteten (Sur l'electricité atmosphérique)*, par H.-E. Hamberg. Upsala, 1872.

in consequence perpetually goes on increasing. The forces tending to conduct the electric molecules in the vertical and horizontal directions are very feeble above and around the magnetic poles; and consequently the electric density of the atmosphere attains a maximum at a certain distance from those poles. As we have seen, this maximum of electric density forms a zone enclosing, in the northern hemisphere, both the magnetic and the astronomic pole; and doubtless it is the same in the southern hemisphere. The magnetic forces act always with equal intensity, as the relatively slight variations undergone by the terrestrial magnet from one period to another can be neglected. If, then, the conductivity of the air were equally invariable, the electrical tension of the lower strata of the atmosphere would remain always the same; but as this condition is by no means fulfilled, that tension must necessarily vary. It is evident that the electric tension at a given point near the surface of the earth does not depend solely on the conductivity of the air around that point, but also on that of the higher strata, up to the height where the conductivity becomes sufficiently good in consequence of the rarefaction of the air. Suppose, for example, that a fog envelops the terrestrial surface, and that above the fog is a stratum of dry air impenetrable to electricity; the force of magnetic induction, still active, will then direct the electric molecules from the earth into the fog, which will soon show itself electropositive, as observations have proved. If, on the contrary, the fog extended up to the higher, conductive strata of the air, doubtless the electric charge of the lower strata would disappear suddenly. From this we see how hazardous it is to attribute the greater or less electric charge of the air at a given place to the greater or less humidity of the air observed at the same place. In my opinion it is highly probable that the periodic variations, both the diurnal and the annual, in the electric state of the lower strata of the air have their cause in the variations of the conductivity of the atmosphere; but certainly it is not enough to take into consideration only those variations which happen upon the spot where the observations of atmospheric electricity are made.

The electric condition of the air in the polar regions is especially interesting. Scoresby, in spite of reiterated trials, found it impossible to discover in those regions the slightest trace of electricity in the air*. The French expedition of the corvet 'La Recherche,' which passed the winter of 1838–39 at Bossekop in the Altenfjord (about 70° N. lat.), applied themselves repeatedly to the examination of the electricity of

* An Account of the Arctic Regions, vol. i. p. 382: Edinburgh, 1820.

the air. MM. Lottin and Bravais, who made during the summer experiments of this kind in lower latitudes, obtained positive deflections upon a straw electroscope when it was placed in metallic connexion with arrows shot into the air. Now these deflections ceased to be obtained as soon as the latitude of the North Cape was reached. In February and March, however, they several times succeeded in detecting feeble traces of positive electricity in the air on putting the electroscope in connexion with kites which they raised to a considerable height*. During the Swedish expedition to Spitzbergen in 1868, M. Lemström tried in vain to discover traces of electricity in the air (80° N. lat.)†. M. Wijkander had better luck in the expedition of 1872–73 to the same regions. Furnished with a more sensitive apparatus than those employed by his predecessors, in the autumn of 1872 he continually obtained proofs of positive electricity in the air. During the winter, from the middle of January to near the end of May, on the other hand, the electricity shown was sometimes positive, sometimes negative. In fact he obtained 20 positive and about an equal number of negative observations. The observations made in the course of the spring, when the temperature approached zero, gave the same results as the summer observations—namely, slight traces of positive electricity. It is, moreover, a remarkable fact that the air was generally positive during the winter days when auroræ boreales appeared, but on other occasions most frequently negative. Respecting this the following remark is made by M. Wijkander, which in my opinion is well worthy of consideration:—"All the observations which were made agree in this, that, in the latitudes in question, at the highest temperatures the air conducts electricity with great facility—a circumstance to which have been attributed the absence of thunder and the presence of the aurora borealis. Divers physicists have believed that this may be assumed to depend on the humidity of the air in those regions; *but that other causes also contribute to it is proved by the fact that the same temperature and the same degree of humidity do not exert this action to so high a degree in lower latitudes*"‡.

These observations prove indubitably that the cause to which the positive electricity of the air in those regions is due is very feeble. It cannot, in my opinion, be attributed to the humidity and consequent conductivity of the air. If that

* *Voyage en Scandinavie et en Laponie*: *Magnét. terr.*, t. iii.; and verbal communications from M. Siljeström, who took part in this expedition.

† *Översigt af Kongl. Vetenskaps-Akademiens Förhandlingar*, 1869.

‡ *Öfversigt*, 1874.

were the case, fog in lower latitudes ought only to betray insignificant traces of electricity, since the conductivity of fog is surely as good as that of the air of the polar regions under ordinary circumstances. Now we know that, notwithstanding its high conductivity, fog is very strongly electropositive.

The real cause of the above-mentioned results of observation is, in my opinion, that the vertical component of the force of induction, or, in other terms, the force tending to direct the æther (electropositive fluid) from the earth to the air, is very insignificant. The electropositive charge of the air must therefore be feeble, and sometimes so slight that the air becomes negative by communication with the earth, as is shown by M. Wijkander's observations. The earth itself, on the contrary, must always be electronegative in those regions. If no exterior force acted upon the electric fluid, the earth and the atmosphere would be in the neutral state; but as a portion of the electropositive fluid which belonged to the earth has been conveyed into the atmosphere in consequence of the action of the induction-force, the earth itself must be electronegative. The earth being a good conductor of electricity, and at the same time constituting a spheroid, its negative electricity must be distributed in a pretty equal fashion at its surface; consequently the terrestrial surface must likewise show itself electronegative in the polar regions, although the vertical component of the force of induction is there very feeble. With the exception of some rare occasions when it was difficult or impossible for him to detect traces of electricity, M. Wijkander also found that the earth was *constantly* electronegative. It follows also from the proposed theory that the conductivity of the air in the polar regions should, as M. Wijkander describes, appear greater than at lower latitudes for the same temperature and with the same humidity. If a conducting body placed in the atmosphere is charged with electricity, evidently it must be influenced by the vicinity of an electronegative body so large as the earth. If the conductive body is charged with positive electricity, this electricity is attracted downward by the negative earth; if the charge is negative, the reverse takes place, for the same reasons. Consequently, in both cases the loss of the electricity in the air is accelerated by the negative earth being in the vicinity of the charged body. Now the force of induction of the earth acts upon the electricity of the body in the opposite direction. If the body is electropositive, that force tends to direct the electricity of the body from below upwards, and *vice versâ* if the body is electronegative. Therefore the earth's induction-force tends to diminish the influence of the earth's electricity upon

the charge of the body. In the regions of the terrestrial surface where the force of induction is either zero or very little, the body will in consequence more readily lose its electric charge than in the localities where that force is greater; the conductivity of the moist air will therefore *appear* greater in the polar regions than at lower latitudes. The composition of pure air is, without any doubt, the same in the frigid as in the temperate zone; at the same temperature and under the same pressure it must in both regions contain, when saturated, the same quantity of aqueous vapour: and it is impossible to discover any reason for which its electric conductivity should be different. It is, therefore, to exterior causes that we must attribute the rapid loss of the charge of electrified bodies in the polar regions; and the exterior causes are probably those which have just been indicated.

According to the proposed theory it is self evident, without any other explanation, that the air would, as was proved by the observations of M. Wijkander, exhibit traces of positive electricity on the days distinguished by intense auroræ boreales.

The cause just given for the electric state of the atmosphere is probably the only one which acts uninterruptedly everywhere; but no doubt there exist others, the action of which, connected with certain localities, is of a more accidental character. To these belong, for instance, the development of electricity described by M. K.-A. Holmgren, who found that, on the division of a liquid into drops, an electromotive force arises at the point itself where the division is effected *. To the same force is probably due the negative electricity of the fine drizzle carried away by the air in cascades or powerful cataracts. As to evaporation, vegetation, the friction of the molecules of the air against one another or against the surface of the earth, as well as several other phenomena in which some have been willing to trace the cause of the electricity of the air, they have assuredly no sensible influence upon the phenomenon in question †. Of course, however, if clouds in the

* "De l'Électricité comme Force cosmique," par K.-A. Holmgren, *Mémoires de l'Académie des Sciences*, t. xi. (1872).

† Of all those so-called causes of the electricity of the air, and of the aurora borealis, evaporation is, without doubt, that which has attracted most notice; but manifold reasons may be cited for the opinion that evaporation has nothing to do with this phenomenon. We may observe, in the first place, that no one has ever succeeded in definitively proving, by experiments made in the laboratory, that evaporation produces electricity. This opinion, therefore, is not founded on a solid basis of experiment. Further, according to this opinion the electricity of the air ought to be more intense in summer than in winter, seeing that the evaporation is

atmosphere have been charged, in the way indicated, with positive electricity, other clouds may in turn be negatively electrified by influence.

As was said above (p. 364), the electropositive fluid (the æther) flows from the upper strata of the atmosphere to the earth in the direction of the dipping-needle. The vertical component of the induction-force in general diminishes as we remove from the equator towards the pole, while the density of the electric fluid present in the atmosphere increases with the latitude. On arriving near enough to the pole for that component and the electric resistance of the air to be no longer capable of opposing a sufficient obstacle, the positive fluid flows down into the electronegative earth. The localities where this takes place form a continuous zone surrounding, in the northern hemisphere, both the magnetic and the astronomic pole, and descending in America to lower latitudes than in the Old World. In my opinion, to the passage of these currents through the rarefied air we must ascribe the production of the aurora borealis*.

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greater in the former season than in the latter. Now, as every one knows, what takes place is precisely the contrary. The positive electricity produced by evaporation, chiefly in the torrid zone, would rise into the atmosphere with the ascending aqueous vapour, would then be conducted by the upper currents of air (the counter-trades) towards higher latitudes, where it would form the aurora borealis by its descent to the earth. But these currents are at an insignificant height from the ground in comparison with that of the auroræ boreales; and, besides, they descend to the surface of the earth long before arriving at the regions marked by the principal frequency of these phenomena. Although complete confidence cannot be accorded to the measurements of the altitude of the aurora borealis, we are certain that it is sometimes very considerable. Some determinations made during the above-mentioned French expedition indicate a height of 150 kilometres; and the height has been found still greater on other occasions. (At the highest latitudes, however, the aurora borealis may from time to time appear at a short distance from the terrestrial surface, as is proved by the observations of Farquharson, Wrangel, Parry, Lemström, and several other arctic voyagers.) At so great an elevation the extremely rarefied air is assuredly not troubled by winds; and it is difficult to conceive how that rarefied air could become electric through the evaporation produced in the equatorial regions. If there existed no special force to raise the electropositive fluid into the atmosphere, this fluid must immediately descend to the earth, and the atmosphere would certainly exhibit no traces of electricity.

* On the passage of electric currents through the air M. Lemström has made some experiments which throw much light upon the phenomenon in question (*Archives des Sciences Phys. et Nat.* t. liv. pp. 72, 162). With the aid of a Holtz machine he kept at a determined electric charge a metal knob furnished with some metallic points. With this view the knob was connected by a conducting wire to one of the poles of the machine, while the other pole was in communication with the earth.

It is evident, therefore, that auroræ boreales must become more numerous in proportion as we come nearer to this ring from the south, and that their greatest frequency will be under this ring itself, while they commence to decrease again in number and in brightness at still higher latitudes. Southward of the ring, the observer sees the aurora in the north ; if he is beneath it, the aurora occupies, when seen under favourable circumstances, the greater part of the sky ; and, lastly, if he is northward of the ring, the aurora appears on the south. It can hardly be admitted that this ring occupies an absolutely fixed position in the atmosphere ; rather is it probable that on one occasion it is situated more to the south or more to the north than on another—a circumstance which may depend on the modifications of the electric conductivity of the terrestrial atmosphere. If, then, the place of the observer is at a point of the earth's surface over which the ring is usually situated, he may see the aurora borealis sometimes to the north, sometimes to the south. If this ring formed a true circle with the magnetic pole for its centre, if the intensity of the descending current were the same at every point, and consequently produced everywhere the same intensity of light, an observer on the earth to the south of the ring would necessarily see the crown of the boreal arc in the plane passing through the place of observation, the centre of the earth, and the magnetic pole. Now, if the declination-needle placed itself entirely in this plane, one would, in consequence, always perceive the summit of the boreal arc in the plane of terrestrial magnetic declination. But the ring in question does not form a perfect circle, nor can we assume that the descending currents possess everywhere the same luminous intensity. Besides, the plane in

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At a certain distance from the knob some Geissler tubes were fixed to an insulated stage permitting them to be brought near to or moved away from the knob. The tubes were, as usual, furnished at their extremities with thin platinum wires. The posterior extremities were connected with the earth by a conducting wire, while the anterior extremities, or those turned towards the knob, were insulated in the air. Although there was no metallic communication between the tubes and the knob, they nevertheless commenced to be luminous as soon as the machine was put in motion, and that even when the distance between the tubes and the knob rose to 2 metres. The current which produced the luminous appearance must therefore have traversed a length of 2 metres through a stratum of air of ordinary density. It was natural that no luminosity was produced in this layer of dense air. These experiments appear to me to have much analogy with the phenomena produced on a grand scale at the formation of the aurora borealis in the terrestrial atmosphere: the electric currents coming from the upper strata descend into the earth without producing luminous phenomena in the lowest strata of the atmosphere.

question indicates only approximately the direction of the declination-needle. From theory, then, it follows that the direction of the declination-needle must indicate generally and approximately the summit of the boreal arc, though we are not authorized to maintain that the two must entirely coincide with one another.

In the localities on the terrestrial surface situated beneath the ring of maximum electric density the electricity descends in the direction indicated by the dipping-needle of the locality; for, as we have demonstrated above (p. 364), the action of the inductive force of the earth is equal to zero in that direction.

The descending currents cannot be compelled to deviate from the above-mentioned direction, unless, in consequence of an accidental meteorological state of the atmosphere, the electrical resistance of the air is greater in that direction than in another, in which case the intensity of the descending current will be most considerable in the direction of least resistance. As a case of this kind may easily occur, we are authorized to maintain, on the ground of the theory, only that the descending currents must be *in general* parallel to the dipping-needle. Now the current betrays its path through the air by a line of light. If those luminous lines parallel to one another be viewed from the surface of the earth, they will appear to converge to a point, by the same laws of perspective to which is due the visual convergence of the rows of trees of a long avenue. As to the point, it will be found in the direction which the dipping-needle at the place of observation indicates in the sky. To this optical phenomenon is due the auroral crown which appears in complete auroræ boreales. The currents in question are formed as soon as the difference of electric tension between the atmosphere and the earth is great enough to surmount the obstacle presented by the resistance of the air. Now, the earth being a good conductor of electricity, and constituting a sphere, its negative charge must be nearly the same everywhere. Doubtless no one will maintain that the positive charge of the air in the southern hemisphere is constantly equal to the same charge in the northern hemisphere. The forces which tend to render the air electropositive and the earth electronegative are equal in the two hemispheres; but the result of the activity of those forces depends in part on the meteorological state of the air, which may be different for each hemisphere. Nevertheless, as we have said, there cannot be any great difference in the negative charge of the earth; and consequently one at least of the causes on which the discharge depends is common to both hemispheres. Therefore nothing very extraordinary can be found in the fact that

auroras are often simultaneous in the northern and the southern hemisphere.

If we compare the theoretic deductions above formulated with the results of observation, we shall find that there exists a satisfactory accordance between them.

According to Loomis, in North America, under the meridian of Washington and at the 40th parallel, 10 auroras per year are seen; under the 42nd parallel the number amounts to 20; and near the 45th the number is 40. In the latitude of 50°, the number of auroræ boreales yearly is stated at 80; and between this latitude and that of 62° the aurora appears nearly every night. Between the last two latitudes the aurora borealis appeared quite as often to the south as to the north. Here, then, is situated the zone, properly so called, of the auroræ boreales, of which about 56° may be considered the mean latitude. To the north of 62° the auroras appear almost exclusively on the south side, and they diminish in number and brightness as we advance northward. At 67° lat. their number has fallen to 20, and is only 10 in the vicinity of 78°. The same fact presents itself at the meridian of St. Petersburg; but here the zone of the auroras is situated much more northward than in America: it is only between the 66th and 75th degrees of north latitude that the annual number of auroræ boreales is stated at 80*.

A multitude of measurements upon the position of the auroral arc were made at Bossekop in the winter of 1838–39, by the French Expedition to Spitzbergen and Norway. The result of more than 200 measurements was, that the crown of the arc was situated 10° west of the magnetic meridian. Argelander had arrived at results nearly the same by his observations made at Åbo, in Finland. In consequence of accidental circumstances, the crown of the auroral arc appeared several times to the east of the magnetic meridian. The position indicated being the mean of all the observations, is cleared of accidental perturbations†. The fact that the crown of the auroral arc must in those regions appear, on the average, to the west of the magnetic meridian flows directly from the theory, if we take into consideration the geographical situation and the magnetic declination of the locality, as well as the form and situation of the annular space of the maximum of electric density. It is easy, in the same way, to understand the accuracy of Argelander's observations. In North America, on the contrary, as in Siberia, the crown of the auroral arc must more nearly coincide, on the average, with the magnetic meridian;

* Loomis, Annual Report of the Smithsonian Institution, 1866.

† *Sur les Aurores boréales vues à Bossekop et à Juprig*: Paris, 1846.

I am not, however, acquainted with any results of observations, freed from accidental perturbations, of a nature to confirm or refute this assumption.

Wilcke* had already observed that the place of the auroral crown is in the zenith of the magnetic meridian, or in its vicinity. The correctness of his observations has been many times confirmed since then by other physicists. The above-mentioned French Expedition made 43 determinations, the mean result of which was, that the situation of the crown deviates less than 1° from the magnetic zenith. The difference, however, between the two positions amounted to 15° on one occasion, and to 12° on two others.

In explaining the annual period of frequency of the auroræ boreales shown by the observations, the following circumstances must be considered:—The electric fluid accumulated in the earth's atmosphere by the unipolar induction of the terrestrial magnet descends to the surface of the earth either by disruptive discharges (thunder-storms), or in feeble continuous currents. The former have their principal frequency between the tropics, and the latter in high latitudes. The fluid which does not flow to the earth in the first of these two ways, is conducted by the magnetic force to higher latitudes, where it flows down in the form of continuous currents. From this it follows that the rarer and weaker the tempests, the more intense and numerous must the auroræ boreales be, and *vice versâ*. In the zone of calms, immediately to the north of the equator, thunder is heard throughout the year; but the limits of this zone vary from one season to another. Outside of that zone, but between the tropics, thunder-storms travel, like the rainy seasons, with the sun. We can therefore assume that the quantity of the electric fluid which within the tropics descends to the earth in tempests is not the same all through the year. As is known, the auroræ boreales present two maxima, viz. at the spring and autumn equinoxes. According to our view the tempests would consequently be the weakest, or, rather, the least quantity of electric fluid would descend in lightning from the atmosphere to the surface of the earth, within the tropics, when the sun crosses the equinoctial line. We have not sufficient materials of observation for deciding whether this view is correct or not†.

It cannot be admitted that the electric fluid flows down upon the earth only on the occasion of auroræ boreales. Beyond all doubt the flow is continual, although mostly the currents do

* *Kongl. Vetenskaps-Akademiens Handlingar för* 1777 (vol. xxxviii.).

† It appears to follow from the observations made upon the aurora borealis by the Austro-Hungarian Arctic Expedition of 1872–1874, that

not possess the force to render the air luminous. The electric fluid is driven into the air, by forces incessantly active, from every point of the earth from the equator to the localities where the downflow is effected; and that fluid is at the same time conducted from the lower to the higher latitudes. We must therefore admit the incessant passage of currents from the equator towards the poles, while the electric fluid circulates in the opposite direction within the earth. This does not mean that the direction of these currents is entirely north and south, many causes contributing to make them deviate from that direction. We picture to ourselves the atmosphere cut by a plane parallel to the equatorial plane and situated between that plane and the auroral ring. The electric fluid driven by the active forces of the earth into the atmosphere between the equatorial plane and the plane in question must then pass through the latter. The sum of the currents passing through a plane of this kind will therefore be greater in proportion as the latitude of the plane is higher; consequently the intensity of the currents increases from the equator towards the poles. Although the electromotive forces to which these currents are due are always the same, yet their intensity must be subject to incessant variations, seeing that it of course depends also on the resistance they meet with in their course. This resistance must depend in great part on the constitution of the air in its lower strata: when these are saturated with humidity, the electric resistance is much less than when they are relatively dry. As, for this reason, these primitive currents often vary in intensity, induction currents will result, of a sort to complicate still more the system of currents we are here considering.

It is obvious that these currents must act upon a declination-needle placed at the surface of the earth; but to calculate the intensity and direction of the action is not so easy. In the first place, the currents in question act directly on the declination-needle almost in the same manner as the current which passes through the circuits of a galvanometer acts on the needle of that instrument; and, in the second place, it must be remembered that the earth contains a quantity of magnetizable materials, the magnetic condition of which is modified

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the zone of maximum of auroræ boreales shifts so as to be found more northward during the winter and summer than at the periods of the autumnal and vernal equinoxes. If this be confirmed by future observations, the annual variation ascertained at lower latitudes may be accounted for by this displacement. (See *Nordlichtbeobachtungen der österreichisch-ungarischen arktischen Expedition* 1872–1874, by C. Weyprecht: Vienna. 1878.

by these currents. All these circumstances have a marked influence upon the declination. Thus, although it may be impossible beforehand to determine exactly the action of these currents upon the declination-needle, we can compare the variations of the declination with the results given by the observations.

The relative humidity of the air is in general greater during the night than during the day. It may be taken as a rule that the diurnal variation of that humidity increases with the variation of its temperature: it is consequently greater in summer than in winter. The conductivity of the air, and, in consequence, also the intensity of the currents in question, have therefore a diurnal and an annual period. Now, if it be admitted that the diurnal variations of the declination depend chiefly on the variations in the intensity of the currents we are considering, it follows that the diurnal variations of the declination should be greater in summer than in winter, and should moreover increase with the distance from the equator—a deduction confirmed, as we know, by the observations. To this may be added that the action upon the declination-needle does not depend exclusively on the nature of the air and the intensity of the current at the locality in which the needle is placed; the intensity of the currents in localities the most distant acts also, although in a less degree. Besides, it is evident that here we have to do with the humidity of the air not merely at the surface of the earth, but also in the upper strata of the atmosphere. We ought not, then, to expect that the variations of declination at a given place will be in direct proportion to the humidity of the air at the same place.

When, for one cause or another, the current descending to the earth from the upper regions of the atmosphere has acquired sufficient intensity, it produces a luminosity in the rarefied air, and we then have the aurora borealis. If the current is endowed with an invariable intensity (as appears to be the case in some of the feebler auroræ boreales), the needle remains pretty steady; but when there are rapid variations in the intensity of the current, or when its maximum of intensity shifts from one point to another (in which case the aurora becomes sparkling and changes its aspect continually), the needle, as can be readily understood, becomes agitated and moves violently. This agitation extends over a considerable portion of the earth's surface, and thus indicates what is going on in the atmosphere, even when no aurora borealis is perceived.

The thesis that the auroræ boreales are produced by electric currents is not new; the majority of physicists have long been agreed in regard to this, with good reason. But hitherto no

one has been able to discover the true and most active cause of those currents, as well as of the electrical phenomena of the atmosphere in general. In fact there are good grounds for asserting that none of the explanations given, up to the present, of these phenomena will bear the examination of scientific criticism. If, till now, no one has seen in these phenomena the results of the unipolar induction of the earth, it is, without any doubt, because an idea has been formed of the nature of that induction which did not permit its application to this object.

There still remain many things that are obscure in the phenomena of the aurora borealis and atmospheric electricity. Of these it will be sufficient to indicate the secular periods in the frequency of auroræ boreales. The relation of these periods with the solar spots gives positive evidence of the cooperation of extratellurian forces. The preceding statement has no claim to be presented as a complete theory of atmospheric electricity and auroræ boreales. My intention has been simply to show that the unipolar induction of the earth plays a most important and significant part in the explanation of those phenomena, and that it ought not to be neglected by those physicists who hereafter apply themselves to this matter.

LV. *On a Test for Telegraph Lines.*

By Oliver Heaviside*.

THE true conduction and insulation resistances of a uniform line may be found from the potential and current at the ends, when a constant electromotive force acts at one end. Suppose at one end A of the line there is a battery of electromotive force E, and a galvanometer, the two together of resistance R_1; also at the other end B of the line a galvanometer of resistance R_2, the circuit being completed through the earth. If the potential at distance x from A, where $x=0$, is v, the current at the same point γ, the conduction and insulation resistance k and i respectively per unit of length, then

$$\frac{d^2v}{dx^2} = h^2 v,$$

where

$$h^2 = \frac{k}{i};$$

and

$$\gamma = -\frac{1}{k}\frac{dv}{dx};$$

* Communicated by the Author.

whence

$$\left.\begin{aligned} v &= ae^{hx} + be^{-hx}, \\ \gamma &= -\frac{1}{\sqrt{ki}}(ae^{hx} - be^{-hx}), \end{aligned}\right\} \quad . \quad . \quad (1)$$

where a and b are undetermined constants.

If now the potential and current at A are v_1 and γ_1, and the same at B are v_2 and γ_2, then it may easily be shown from equations (1) that

$$ki = \frac{v_1^2 - v_2^2}{\gamma_1^2 - \gamma_2^2}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

Since the length of the line does not appear in (2), the relation therein expressed applies to any two points of the line. The reason is that the product of the conduction and insulation resistances is the same for any length, the one varying directly and the other inversely as the length. Now the insulation of land-lines is in this country very variable, while the real conduction resistance (*i. e.* its resistance if it were perfectly insulated) is nearly constant. It follows that (2) may be used for determining i, considering k as constant. In (2),

$$\left.\begin{aligned} v_1 &= \mathrm{E} - \mathrm{R}_1\gamma_1, \\ v_2 &= \mathrm{R}_2\gamma_2. \end{aligned}\right\} \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

R_1 and R_2 being interposed resistances are, of course, known; so that three quantities have to be observed, viz. E, γ_1, and γ_2; or equivalent information must be obtained. To make the test in its simplest form, let the resistances R_1 and R_2 be small compared with the line resistance. Also, let equally sensitive tangent-galvanometers be used, and let n_1 and n_2 be the deflections corresponding to γ_1 and γ_2, and n_3 the deflection E gives through 1000 ohms. Then (2) becomes

$$ki = \frac{n_3^2}{n_1^2 - n_2^2} \times 10^6, \quad . \quad . \quad . \quad . \quad . \quad (4)$$

where k and i are both in ohms; or if k is in ohms and i in megohms, the 10^6 must be cancelled.

If R_1 and R_2 are taken into account, then instead of (4) we have

$$ki = \frac{(10^3 n_3 - \mathrm{R}_1 n_1)^2 - (\mathrm{R}_2 n_2)^2}{n_1^2 - n_2^2};$$

and if the galvanometers are not equally sensitive, the deflection n_2 must be multiplied by the ratio of the sensitiveness of the galvanometer at B to that at A.

Using formula (4), the test can be easily made, though it is

obvious that the line must be long enough to make an appreciable difference between the sent and received currents.

We may also determine k and i separately from the same data. If l is the length of the line, then

$$\left.\begin{aligned} kl &= \sqrt{ki}\log\frac{v_1+\gamma_1\sqrt{ki}}{v_2+\gamma_2\sqrt{ki}},\\ \frac{i}{l} &= \sqrt{ki}\div\log\frac{v_1+\gamma_1\sqrt{ki}}{v_2+\gamma_2\sqrt{ki}}. \end{aligned}\right\} \quad . \quad . \quad . \quad (5)$$

It is to be observed that these formulæ give the true conduction and insulation resistances. The measured resistances, or those deduced from observations with the bridge, differential galvanometer, &c., at the battery-end alone, are very different from the true, when the line is long and badly insulated. The measured is always less than the true conduction resistance, and the measured always greater than the true insulation resistance; while the measured conduction resistance can never be greater than $\sqrt{ki}$, and the measured insulation resistance never less.

LVI. *On Electrical Discharges in Insulators.* *By* Dr. W. C. Röntgen, *Professor of Physics in the University of Strasburg**.

IN the following communication are contained the results of an experimental investigation begun long since, but often interrupted, on the disruptive discharge of electricity through insulators; for I had set myself the task to discover whether in such a discharge there exists any expressible relation between the physical constitution of the insulator, the difference of potential required for a discharge, and the quantity of electricity discharged. The investigation extended to solid, liquid, and gaseous bodies; but up to the present time I have only succeeded with the latter in finding such a relation.

The solid bodies, mostly crystals, were placed, in the form of thin plates, between two rounded-off brass points, one of which was led away to earth, the other connected with a source of electricity, mostly a Holtz machine. By slow rotation of the machine the potential was raised until a spark passed through the thin plate. An electrometer specially constructed for the present case permitted the course of the potential to be

* Translated from a separate impression, communicated by the Author, from the *Nachrichten der Kön. Gesellschaft zu Göttingen*. 1878.

traced, and the potential itself to be accurately determined at the moment of the discharge. I hoped in this way to obtain, with plates of different substances and especially with plates cut in different directions out of the same crystal, a characteristic difference of potential for each substance and for each direction; but hitherto all my endeavours have been fruitless. I found it impossible, with one and the same plate, to obtain satisfactorily accordant values from the different experiments of one and the same series. The cause of this irregularity is doubtless to be sought in an unavoidable difference in the disposition of the electricity on the points and the plate. The difference of potential necessary for a spark-discharge is essentially dependent on this disposition, which, in the method of experiment chosen, changes before the spark passes, in consequence of a less or greater conductivity of the plate and its surface, as well as in consequence of electricity added by convection from the point in an irregular and uncontrollable manner. Perhaps, if experiments were made with much larger plates and with very obtusely pointed electrodes, more favourable results might be obtained.

The experiments which I made with liquids are, notwithstanding their number, still too imperfect, and offer too few general points of view, for their details to be communicated.

As is well known, electrical discharges in gases have often been the subject of investigation; both the spark-discharge with greater and less pressures and also the slow discharge known by the name of dissipation have been repeatedly examined. From these experiments no simple relation can be with certainty deduced between any constant of the different gases and the difference of potential corresponding to each gas, necessary for a discharge, or the amount of electricity discharged. Yet it would be hazardous to conclude, on the ground of those experiments, that such a relation does not exist. For, in the first place, with spark-discharges it is always to be feared that the decomposition which doubtless takes place in some gases, as well as the considerable alteration of temperature in the path of the spark, may possibly conceal any such relation; and, secondly, some hitherto unpublished experiments made by M. Warburg have shown that in gases dissipation cannot with certainty be demonstrated; while the loss of electricity by conductors which are insulated in gases, observed by Coulomb, Riess, Warburg, and others, is very probably brought about only by the insulating supports and by particles of dust*.

I therefore, after numerous preliminary experiments and

* See Boltzmann. Pogg. *Ann.* vol. clv. p. 415.

mature reflection, resolved to select for my purpose a kind of discharge which has hitherto been but little studied, viz. the so-called discharge by convection, such as is known to take place between a very sharp point and a large smooth plate. I believe I may indeed attribute it to this selection, if I finally succeeded in discovering the relation sought.

The method of experiment at last found serviceable was the following. A Holtz machine was kept in action with as constant and high velocity of rotation of its disk as possible, by means of a Schmidt water motor. One of the electrodes was connected with the earth by the gas-pipes; and from the other a copper wire covered with gutta percha led to the inside coatings of two Leyden jars constructed according to W. Thomson's plan, of well-insulating glass and containing sulphuric acid, the outside coatings of which were led away to earth. These jars formed an electrical reservoir of considerable capacity, and were intended to diminish as much as possible the variations of potential which might be occasioned by an irregular development of electricity by the machine. Behind these jars the wire was divided. One branch went to a narrow glass tube filled with glycerine, which served as a rheostat; by lowering into or drawing out of it a metallic conductor to earth, the resistance of the glycerine could be fixedly diminished or augmented. The other branch led first to the point in the discharge-apparatus, and from that to an electrometer constructed expressly for the investigation.

The discharge-apparatus consisted of the following parts:— A vertical brass rod, provided beneath with a gilt sewing-needle, passed well insulated through the tubular neck of a glass bell, which was fitted air-tight upon the plate of an air-pump. In the space enclosed by the bell stood, carefully insulated from the plate, at a distance of 19·3 millims., with its centre opposite the point, a polished brass disk of 132 millims. diameter; this was in conducting connexion with one end of the coiled wire of an extremely delicate mirror-galvanometer consisting of a great number of turns; the other extremity of the wire led to the gas-pipes. By an air-pump and further suitable arrangements the bell could be filled with different gases at various pressures determined by a manometer.

The electrometer made use of proved, it is true, serviceable for the present investigation; but it still has some defects, which must be removed; I am therefore engaged in constructing a better apparatus, of which I hope subsequently to give an account. I will only mention further that it was arranged after the manner of Thomson's quadrant-electrometer, and that

the readings were reduced to comparable measure by comparison with a long-range electrometer which I had made for the most part according to Thomson's description.

It was found that 6 of the units in which in the following the differences of potential are expressed correspond to a potential-difference of about 5 Daniells. I would not, however, lay too much stress upon these data, since the battery at my disposal was too small to enable me to accomplish a more accurate determination.

Now, if we assume that the Holtz-machine electrode which is connected with the gas-pipes conducts away negative electricity, the positive electricity issuing from the other electrode finds two paths—the first through the rheostat to the gas-pipes, and the second through the discharge-apparatus and the galvanometer, likewise to the gas-pipes. The quantity of electricity which passes through the discharge-apparatus can now be varied within wide limits by altering the resistance of the rheostat. The galvanometer indicates this quantity; and the electrometer measures the difference of potential between the point and the plate.

I soon observed that the discharge does not take place with every difference of potential, but that rather a perfectly fixed difference is always necessary in order to induce it. If at the commencement of the experiment the resistance of the rheostat has been made nearly $= 0$ (when of course the deflections of the galvanometer and electrometer are likewise $= 0$), and if now the resistance be gradually increased, on the electrometer indeed a steady rise of the potential will be observed, but the potential must have reached a certain value before the galvanometer will show, by a sudden, proportionally great, and constant deflection (if the resistance of the rheostat remains invariable), that the discharge has commenced. When once the discharge is present the resistance of the rheostat and consequently the potential can be again diminished, through which the discharge, it is true, steadily decreases, but does not at once sink to zero. Only with a considerably less potential-difference than that with which the discharge commenced does it again entirely cease.

I found, further, that the commencement of the discharge was dependent on many collateral circumstances, *e. g.* whether a discharge had taken place a shorter or a longer time previously; unavoidable dust particles, too, have probably an influence. On the other hand, determinations of the difference of potential at which the discharge ceases, derived from different experiments separated from one another by considerable

intervals of time, gave values which agreed excellently with each other. I resolved on this account, at least preliminarily, to direct my attention chiefly to the determination of this difference of potential, which we will name the minimum potential-difference, and, for brevity, denote by M. P.

The moment when the discharge ceases is, for the most part, characterized by this—that the already much diminished galvanometer-deflection (amounting to only 2–4 scale divisions), after a further very slight lessening of the resistance in the rheostat, suddenly becomes zero. At this instant the M. P. is read off at the electrometer. I am inclined to account for this phenomenon by the small variations which the potential undergoes notwithstanding the insertion of the Leyden jars. The electrometer, which is provided with a powerful damper, gives the mean value of the variations of the potential. The fact that the discharge had now really ceased I verified also in another way: that is to say, if the galvanometer was made considerably more sensitive by being rendered more perfectly astatic, its deflection vanished at exactly the same difference of potential as before; in like manner an electroscope, which instead of the galvanometer was connected with the plate in the discharge-apparatus, was not charged, and the characteristic star-shaped luminous appearance visible in the dark, which was present during the discharge, disappeared when the M.P. was attained.

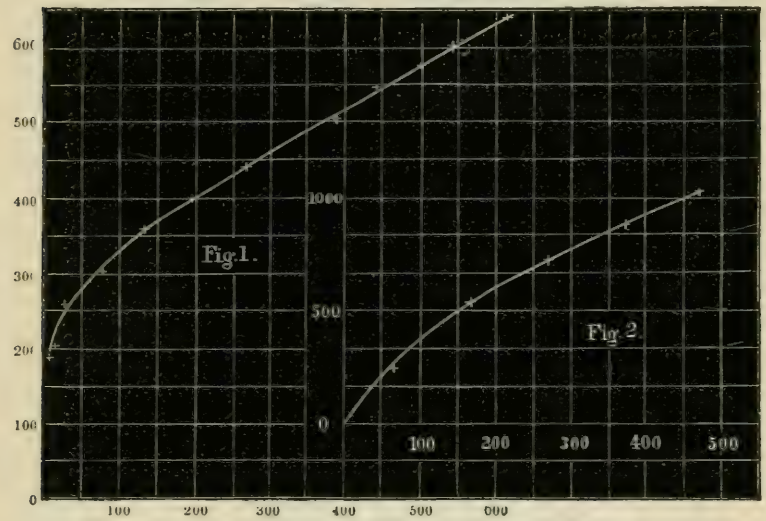
In all the following experiments the distance of the point from the plate remained the same. Further, the temperature was constant, at least in those experiments which were to be compared with one another; and, lastly, it is to be noticed that the point was always positive when the contrary is not expressly stated.

Unfortunately, the investigation had to be interrupted, first because the seasons of spring and summer are very unsuitable for working with static electricity, and secondly because for its continuation the reconstruction of some of the apparatus, especially of the electrometer, had become absolutely necessary. Consequently, of the many questions which might be put, only a few can be answered. The results are given below.

1. How does the M. P. in a gas depend on the pressure? The question was repeatedly answered for dry air free from carbonic acid. Fig. 1 represents the result of one experiment: the pressures in millims. of mercury were laid down as abscissæ, and the M. P. as ordinates. The unit in which the latter are expressed is not directly comparable with that mentioned above.

Pressure in millims. of mercury	615	544	499	445	385	266	198	138	68	29	10·9	7·1
M. P.	639	602	577	547	503	439	402	361	301	258	198	189

It follows from these experiments that with pressures above 200 millims. the increase of the pressure is at least nearly pro-



portional to the increase of the M. P.; below that limit the M. P. diminishes much more quickly in proportion. Similar ratios were found with other gases.

2. How, in a gas which is subjected to a fixed pressure, is the quantity of electricity discharged connected with the difference of potential between the point and the plate?

Dry air, free from carbonic acid, was tried with the pressures 391, 294, 203·4, 109·7, and 51·8 millims. mercury. The highest difference of potential which could be determined with my electrometer was 3684 units (6 units = 5 Daniells); the greatest quantity of electricity that could be measured amounted to something over 500 arbitrarily chosen units. The following Tables contain in the first column the differences of potential, in the second the corresponding amounts of electricity discharged; and in the third I have given, under the name of "disposable potential-differences," the differences between the numbers in the first column and the M. P. corresponding to each pressure, the quantity of electricity discharged being of course =0. I have calculated these differences, and given them the name above mentioned, because possibly the view is

correct that the M.P. is compelled to overcome a certain transitional resistance, and that only the disposable potential-difference measures the quantity discharged. The latter shall, for shortness, be denoted by D. P.

Pressure 51·8.			Pressure 109·7.		
Pot.-diff.	Discharge.	D. P.	Pot.-diff.	Discharge.	D. P.
1462	0	0	1806	0	0
1727	71	265	2094	38	288
2004	171	542	2859	208	1053
2199	271	737	3396	370	1590
2349	371	887	3684	522	1878
2487	471	1025			
Pressure 203·4.			**Pressure 294.**		
2162	0	0	2433	0	0
2645	45	483	2859	29	426
2859	67	697	3396	72	963
3396	138	1234	3684	105	1251
3684	192	1522			

Pressure 391.

Pot.-diff.	Discharge.	D. P.
2775	0	0
3169	24	394
3684	65	909

The first of these Tables, corresponding to the pressure 51·8, is graphically represented in fig. 2 ; the abscissæ denote the quantities discharged, the ordinates the D. P. The curves for the other pressures have a similar form.

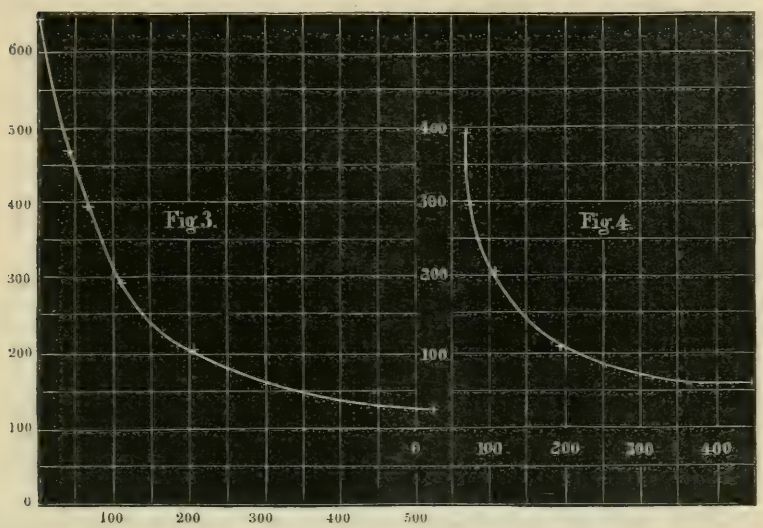
3. In a gas with a determined difference of potential, in what manner does the amount of electricity discharged depend upon the pressure? Dry air, free from carbonic acid, with the potential-difference 3684, was examined in detail.

Pressure in millims. of mercury	641·2	466·4	391·0	294·0	203·4	109·7
Amount of electricity discharged	0	41·5	65	105	192	522

This Table is graphically represented in fig. 3 ; the abscissæ denote the amounts of electricity discharged, the ordinates the pressures. Other gases behaved similarly.

In these experiments, as already mentioned, the difference of potential was constant. But as, according to No. 1, with different pressures the discharge ceases and commences respec-

tively at different potential-differences, the D. P. were not the same; it was consequently still questionable whether any



simple relation subsisted between pressure and electricity discharged, if with different pressures not the absolute but the disposable potential-difference was found constant. The question can be answered from the data of No. 2. I have extracted from the Tables the following comparison, valid for the D. P. $=1000$:—

Pressure in millims. of mercury ...	391	294	203·4	109·7	51·8
Amount of electricity discharged ...	71	79	106	194	450

In fig. 4 will be found the graphic representation. A simple relation is not perceptible. To be sure the product of the pressure into the quantity of electricity for the last four pressures is nearly constant; but with the pressure 391 there is a considerable deviation from this rule. For the purpose of fully answering questions 2 and 3, experiments with different gases, between wider limits of the potential-differences, the pressures, and the quantities of electricity discharged, will be absolutely necessary.

4. Does an expressible relation exist between the minimum difference of potential and the nature of the various gases in which the discharge takes place?

The gases were all tried at two pressures, approximately 205 and 110 millims. mercury; experiments with higher pressures

were excluded, because with some of the gases the electrometer was not equal to measuring the corresponding differences of potential. I must mention that these experiments are not directly comparable with the preceding ones. The following Table contains the mean values of various satisfactorily accordant determinations.

Gases.	M. P. at 205 millims.	M. P. at 110 millims.
Hydrogen	1296	1174
Oxygen	2402	1975
Carbonic oxide	2634	2100
Marsh-gas	2777	2317
Nitrous oxide	3188	2543
Carbonic acid	3287	2655

In this Table the gases are arranged in the order of ascending values of the M. P. If this series be compared with that which is obtained when the gases are arranged in the order of diminishing values of their mean molecular path-lengths, at both 205 and 110 millims. pressure perfect agreement will be found. Since the minimum difference of potential is a direct measure of the insulating-power of a gas, the result contained in the above Table can be expressed in the following manner:—The shorter the path of its molecules, the greater is the insulating-power of a gas. Now it is known that the smaller the gas-molecules the greater is the length of their paths; consequently we can also say:—The larger the molecules of a gas the greater is its power to insulate.

The connexion between the M. P. and the length of path becomes still more convincingly evident when for each gas we form the product of the path-length and M. P.

Gases.	Product of path-length and M. P.	
	Pressure 205 millims.	Pressure 110 millims.
Hydrogen	240	218
Oxygen	254	209
Carbonic oxide	259	207
Marsh-gas	236	197
Nitrous oxide	217	173
Carbonic acid	224	181

The path-lengths are taken from Graham's transpiration

experiments and O. E. Meyer's *Gastheorie*, the factor $\frac{1}{10^4}$ being everywhere omitted.

From these numbers we obtain a remarkable relation: it follows, namely, from both the first and the second series, that the product of the path-length and the minimum difference of potential, measured at equal pressure, has nearly the same value with all the gases investigated.

Stefan pointed out the connexion between path-length and index of refraction; Boltzmann's experiments have shown that the dielectric capacity of gases stands to the index of refraction in the relation required by Maxwell's law; and the present investigation brings the insulating-power of gases into causal connexion with the three above-mentioned properties. Accordingly the insulating-power of a gas is by so much less as the inductive capacity of the gas is greater, and *vice versâ*.

Similar simple relations exist between path-length and M. P. for one and the same gas at different pressures; a simple discussion of the experiments spoken of under question 1 leads to this result.

Besides the gases above adduced, olefiant gas was examined. It was not found to conform to the same law; for the products of the M. P. and the path-length at the pressures 205 and 110 millims. were respectively 149 and 123. I believe, however, that no importance need be attached to this deviation, since the phenomena attending the discharges were of quite a different character from those with the other gases, and permit us almost certainly to conclude that decomposition of this gas took place.

In conclusion, in moist air the M. P., and consequently the insulating-power, was much greater than in dry air.

5. A series of experiments with air and hydrogen prove that, *cæteris paribus*, the M. P. is less when the point is charged with negative than when it is charged with positive electricity; whether the like takes place also in regard to the difference of potential at which the discharge commences I have not yet been able to decide.

Strasburg, May 1878.

LVII. *On the Nebular Hypothesis.—X. Predictions.* By Pliny Earle Chase, *LL.D., S.P.A.S., Professor of Philosophy in Haverford College**.

In accordance with a suggestion of Professor Robert E. Rogers, I endeavoured to find what modes of central force will best represent some of the most general forms of chemical activity, more especially those which are the base of the law of Avogadro and Ampère—of combination by volume, and of approximate constancy in the product of atomic weight by specific heat.

The simplicity of the ratio between the energy of H_2O and the solar energy at Earth's mean distance† furnishes good grounds for such an investigation, while the record of a parabolic orbit connecting the Sun with the nearest fixed stars‡ indicates a proper course for conducting it. Although there may be some doubt as to the degree of certainty which belongs to the recent hypotheses of internal gaseous structure, there can be none as to the graphic representation of orbital activities under forces varying inversely as the square of the distance. Circular orbits denote constancy of relations between radial and tangential forces; elliptic orbits, variability of relations accompanied by cyclical oscillations; parabolic orbits, variability of relations without cyclical oscillations; hyperbolic orbits, variability of relations complicated by the action of extraneous force.

In a rotating mass, the orbits of the several particles are circular. If the uniform velocity of any particle in the equatorial plane is less than $\sqrt{fr}$, the mean action of the central force is impeded by internal collisions or resistances. If the velocities of all the particles in the plane vary precisely as $\sqrt{fr}$, there is a condition of perfect fluidity, marking a limit between complete aggregation and incipient dissociation. Any cyclic variations of velocity between constant limits indicate elliptic orbits, with tendencies to aggregation through collisions near the perifocal apse. A perifocal velocity of $\sqrt{2fr}$ marks a parabolic orbit, and a limit between complete dissociation and incipient association. A velocity greater than $\sqrt{2fr}$ is hyperbolic, indicating the intervention of a third force in addition to the mutual action between the two principal centres of reference.

If all physical forces are propagated by ætherial undulations

* Communicated by the Author.

† Proc. Soc. Phil. Amer. xii. p. 324; xiii. p. 142.

‡ Ibid. xii. p. 523, and subsequent papers.

between resisting points, those points tend naturally to nodal, and from internodal positions. In order to maintain uniformity in the wave-velocity, the ætherial molecules must be uniform, not only in volume, but also in aggregate inertia. As the inertia of the resisting points increases, the inertia due to internal ætherial motions should therefore diminish, and *vice versâ*. In other words, the uniform elementary volume may be represented by the product of atomic weight by specific heat; and the laws of Boyle (or Mariotte), Charles, and Avogadro follow as simple and necessary corollaries.

In order that uniform undulations should produce motion, there must be at least two points of resistance. Those points would approach each other until the interior undulating resistance equalled the exterior undulatory pressures, when their motion would be converted into rotation or into orbital revolution. Their common centre of revolution might become the centre of a new elementary volume, thus giving rise to the various laws of combination by volume, combination without condensation, condensation of two volumes into one, three volumes into two, or four volumes into two, as well as to general artiad and perissad quantivalence.

When perifocal collisions change parabolic or elliptic into circular orbits, there should be increasing density towards the principal centre of the system. Further collisions and condensations would produce tendencies to both nucleal and atmospheric* aggregations, and consequent binary groupings. These laws are exemplified in the solar system by the general division into an intra-asteroidal and an extra-asteroidal belt, and by the subdivision of each belt into two pairs,—the inner belt being denser than the outer, and the inner member of each pair being denser than its companion—Mercury being denser than Venus, Earth than Mars, Jupiter than Saturn, Uranus than Neptune. This arrangement towards the Sun as a principal centre appears, however, to be of more recent date than the tendency to condensation in the Telluric belt; for Earth is denser than Venus, and the great secular ellipticities of Mars and Mercury suggest the likelihood of a quasi-cometary origin. Similar tendencies would contribute to the chemical grouping of atoms by pairs, which is essential for polarity and for the already enumerated laws of chemical combination.

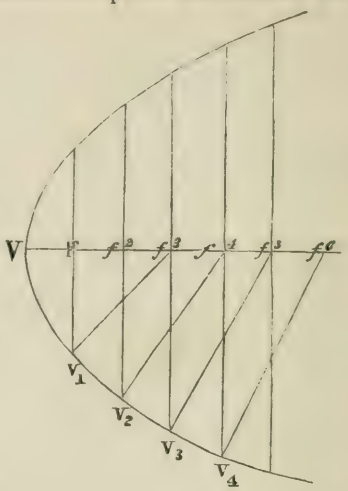
In the "nascent state" particles may be regarded either as parabolically perifocal, with the velocity of complete dissociation from a given centre, or as relatively at rest, and ready to obey the slightest impulses of central force. The mean *vis viva* of a system formed by two such particles would be

$$m \times (\sqrt{2})^2 + m \times 0 = 2m \times 1,$$

* Proc. Soc. Phil. Amer. xiv. p. 622 *seqq.*

representing a change from parabolic to circular orbits and a condensation of two volumes into one.

At the parabolic limit between complete dissociation and incipient aggregation, if the focal abscissæ $x_0 = \mathrm{V\,F}$, is taken as the unit of wave-length, the value of the successive ordinates, as well as the velocity communicated by uniform wave influence acting through the entire length of the ordinates, will be represented by $\sqrt{4x_n}$; the resulting *vis viva*, and the consequent length of path, or major axis, communicable against uniform resistance, by $4x_n$; the successive differences of major axis by 4. Each normal, $v_n f_{n+2}$, equals the next ordinate, $v_{n+1} f_{n+1}$; there are, therefore, triple tendencies, both in the axis of abscissas and on each branch of the curve, to successive differences of 4 in the major axes of aggregation, in consequence of the meeting of abscissal, ordinal, and normal waves in the axis, and the meeting of tangential, normal, and abscissal waves upon the curve. At each node of aggregating collision two of the wave systems are due to normally alternating rectangular oscillations, the third serving as a link between the axial and the peripheral waves. The bisection of the normals by their equivalent ordinates adds importance to the normal major axes, and increases the tendency to aggregation at their respective centres of gravity.



Chemical molecules and atoms are so small that we are unable at present to show so conclusively as in cosmical gravitation that the "nascent" velocity, or the mean radial velocity at the limit between complete dissociation and incipient aggregation, is equivalent to the velocity of light. But the analogies which are here presented are strengthened by the frequent vivid, luminous, and thermal accompaniments of chemical change, and by the electric polarity of combining elements. It seems, therefore, reasonably certain that the same limiting unit of velocity and *vis viva*, which can be easily traced in light, heat, electricity, and gravitation, is also fundamentally efficient in chemical affinity. M. Aymonnet, in his communication of a "Nouvelle Méthode pour étudier les Spectres

Calorifiques,"* says :—" Je ferai remarquer, avant de terminer, que l'étude des spectres calorifiques d'absorption, faite avec des corps portés à diverses températures, peut et doit conduire à la connaissance de lois physiques reliant les phénomènes d'association et de dissociation des corps aux phénomènes calorifiques et lumineux." In another paper recently presented to the French Academy, " Sur le Rapport des deux Chaleurs Spécifiques d'un Gaz "†, M. Ch. Simon deduces the theoretical ratio $C : c :: 1{\cdot}4 : 1$. The first attempt at a solution of the problem upon *à priori* grounds, appears to have been Professor Newcomb's‡, who found from the hypothesis of actual collisions, the ratio 5 : 3 if the particles were hard and spherical, or 4 : 3 if they were hard and not spherical; the second, my own§, based on the general consideration of all internal motions, which led to the ratio 1·4232 : 1; the third, M. Simon's, which took account of rotations and neglected other internal vibrations.

No surer test of any hypothesis has ever been suggested than its furnishing a successful anticipation, or prediction, of facts or phenomena that were previously unknown.

The harmonic progression which starts from Jupiter's centre of linear oscillation as a fundamental unit, and which has 4 for its denominator-difference, was taken as the ground for such a prediction, in the communication which I read to the American Philosophical Society on the 2nd of May, 1873‖. Kirkwood had, a short time before, computed a probable orbit for " Vulcan," which satisfactorily represented the second interior term of the series; and this accordance was one of the principal sources of the confidence with which I ventured upon a publication of the prediction.

Forty-one days afterwards, on the 19th of June, De la Rue, Stewart, and Loewy communicated to the Royal Society certain conclusions, based upon three sets of sun-spot observations, taken in three different years, and extending over periods respectively of 145, 123, and 139 days. Those observations indicated some source of solar disturbance at ·267 of Earth's mean radius vector, which represented the first interior term of my series and gave the first conclusive verification of my prediction. In announcing this fact to the Society, I presented three nearly identical series—the first being determined solely by Jupiter, the second by Earth, and the third by relations of planetary and solar masses¶. I gave precedence to the first of these series, both because of Jupiter's predominant importance, and because of the many planetary harmonies which are determined by Jupiter's mean perihelion**.

* *Comptes Rendus*, lxxxiii. pp. 1102–4, December 4, 1876.
† Ibid. p. 727, October 16, 1876. ‡ Proc. Amer. Astr. Soc. v. p. 112.
§ Proc. Soc. Phil. Amer. xiv. p. 651. ‖ Ibid. xiii. p. 238.
¶ Ibid. pp. 470, 472. ** Ibid. p. 239.

At the time of the late total solar eclipse, Watson and Swift each observed two small planets between the orbit of Mercury and the sun. By comparing the published position of the planet which was first announced by Watson, with some of the most trustworthy of the recorded observations which were thought by Leverrier to indicate intra-Mercurial Transits, Gaillot and Mouchez found an orbital period of 24·25 days*, which represents the third interior term of my series and the second strict verification of my prediction.

If a nebula condenses until all the particles in the equatorial plane have an orbital velocity, the mean radial *vis viva* at the orbital centre of linear oscillation ($\frac{1}{3}r$) is equivalent to the orbital *vis viva* at $9r$. This equality may, perhaps, help to account for the following approximate division of the planetary system, the unit being Sun's radius:—

$9^2r = 81r$. Mercury's mean radius vector..... $83{\cdot}17r$.
$9^3r = 729r$. Asteroid 76 ..... $732{\cdot}42r$.
$9^4r = 6561r$. Neptune's extreme radius vector...$6546{\cdot}67r$.

The relatively rapid motion of Phobos (the inner satellite of Mars), as well as the newly found meteoroidal character of the corona, may reasonably lead us to look for an indefinite number of further verifications among the results of future discovery. Many of them, like many of the Neptunian harmonies which are modified by the overshadowing mass of Jupiter, will probably elude all attempts at discovery; but the apparent importance of 9 as a cosmical factor gives interest to the second series in the following Table. The denominators in the first series are of the general form $(4n-3)$; those in the second series are of the form $(4\nu-3=144n-111)$, ν being equivalent to $9(4n-3)$.

According to the nebular hypothesis, when Sun was expanded to the present orbit of Jupiter, the collisions of subsiding particles would tend to form a ring at two thirds the distance, or at 3·469 times Earth's mean radius vector. If we take this as a harmonic unit, $\frac{1}{1}$, we find that Venus's mean perihelion is well represented by $\frac{1}{5}$, Mercury's mean distance by $\frac{1}{9}$, and Kirkwood's estimated semi-major axis of "Vulcan" by $\frac{1}{17}$. Here are, therefore, four terms of the first harmonic series, with a denominator-difference of 4.

First Series.

No.	Harmonic prediction.			Confirmation.	
1.	$\frac{1}{1}$	=	3·469	Node of subsidence	3·469
2.	$\frac{1}{5}$	=	·694	Venus, mean perih.	·698
3.	$\frac{1}{9}$	=	·385	Mercury, mean d.	·387
4.	$\frac{1}{13}$	=	·267	De la Rue, Stewart, and Loewy	·267
5.	$\frac{1}{17}$	=	·204	Kirkwood	·209
6.	$\frac{1}{21}$	=	·165	Watson	·164

* *Comptes Rendus*, August 5, 1878.

Second Series.

No.	Harmonic prediction.			Confirmation.	
1.	$\frac{1}{33}$	=	·1051	Helios.....	·1060
2.	$\frac{1}{177}$	=	·0196	Themis	·0196
3.	$\frac{1}{321}$	=	·0108	Eunomia	·0108
4.	$\frac{1}{465}$	=	·0074	Phaos.....	·0075
5.	$\frac{1}{609}$	=	·0057	Lychnis	·0056
6.	$\frac{1}{753}$	=	·0047	Sun's surface.....	·0047

The first term of the second series (Helios) represents an orbital node in which the time of revolution would be synchronous with that of a solar half-rotation. This, as I have already said, is equivalent to the time in which the continuous acceleration or retardation of Sun's superficial gravitation would communicate or overcome *the velocity of Light*. In order to maintain equality of areas, the time of rotation in an expanding or contracting nucleus should vary as the square of radius. But g varies inversely as the square of radius; so that gt should be constant at all stages of solar condensation, past, present, or future.

The second term of the series (Themis) represents an orbital node in which planetary revolution would be accomplished in a sidereal day, or synchronously with Earth's rotation on its axis. The closeness of its relation to Earth, and its accordance with the laws of harmony, are both fitly designated by its name—Themis having been regarded as the daughter of Heaven and Earth, and as the goddess of law and order.

The third term of the series (Eunomia) represents an orbital node in which planetary revolution would be accomplished synchronously with Jupiter's rotation on its axis. Its designation has also a double fitness; for Eunomia was the mythical daughter of Jupiter and Themis, her name signifying "good government."

The fourth term of the series (Phaos) represents an orbital node in which planetary revolution would be synchronous with two planetary revolutions at Sun's surface.

The fifth term of the series (Lychnis) represents an orbital node at which Herschel's theoretical "subsidence" would give Sun's present velocity of rotation.

The sixth term of the series represents a node which is somewhat within Sun's apparent surface, or at its actual surface, provided the depth of the photosphere is 1 per cent. of Sun's radius.

The one hundred and eighty-seventh term of the first series represents an orbital node at the upper surface of Sun's photosphere. Its harmonic denominator represents the ratio of Sun's mass to the aggregate planetary mass.

We see, therefore, in the second series, not only the nodal influence of the largest two bodies in the system (Sun and

Jupiter), but also an accordant influence of our own planet, which is the central orb and the largest planet in the belt which is bounded by the secular perihelion of Mercury and the secular aphelion of Mars.

Herschel's modified presentation of the nebular hypothesis, and Gummere's criterion, furnish the needful grounds for a satisfactory explanation of such remarkable velocities as that of the inner moon of Mars. They also seem to require that secondary orbs, when they revolve in less time than is required for the rotation of their primaries, should be denser than the primaries. There is therefore good reason for the further prediction that, whenever the density of Phobos is ascertained, it will be found to be greater than that of the planet itself. If Mars has any other moons which have an orbital period of less than twenty-four hours, they should also be of like superior density. In these harmonies, as well as in many others, the pointing to the primeval organizing agency of light is interesting and suggestive. At the theoretical period of each of the harmonic divisions, and at all other stages of nebular condensation, the rhythmical rotation of our day-star has been repeating its unvarying confirmation of the old, old record. In the dim and distant past, in the living present, and through all coming time, from the great "Beginning," until the culmination of prophecy when "the elements shall melt with fervent heat," nothing but divine intervention has disturbed, or can disturb, the equality between the accumulated action of solar gravity for a half-rotation and the velocity of light. The harmonic hypothesis forecasts the same requirement at the surface of the central orb in every stellar system; so that the closing refrain in the hymn of each of the morning stars is, was, and ever shall be:—

"And God said, Let there be light; and there was light."

LVIII. *On the Light reflected by Potassium Permanganate.* *By* Sir John Conroy, *Bart., M.A.**

THE light reflected from the surface of potassium permanganate was originally examined by Haidinger, who announced (*Sitzungsberichte der kaiserlichen Akademie der Wissenschaften*, Band viii. 1852, p. 133), that when the light reflected from the surface of the crystals and of the substance rubbed on a plate of glass was examined with a dichroiscopic lens, the portion polarized in the plane of incidence was light yellow at low angles, and became white as the angle increased, whilst the portion polarized perpendicularly was light yellow, and became green and blue as the angle increased.

* Communicated by the Physical Society.

Professor Stokes found (Phil. Mag. vi. 1853, p. 400) that the reflected light contained four bright bands, corresponding in position to the dark bands of the absorption spectrum of a solution of the substance, and that when the reflected light was separated into two streams polarized in, and perpendicular to, the plane of incidence, and then examined by a prism, the bands were hardly visible in the one, and the other at a certain angle consisted mainly of them.

E. Wiedemann has recently published (Pogg. *Ann.* cli. 1874, p. 625) an account of some experiments he has made on the same subject. He found that whilst the dark bands of the reflection spectrum did not even partially cover those of the absorption spectrum, they did not lie exactly intermediate between any two of them—and, further, that the position of the bands was independent of the angle of incidence, both with ordinary light, and with that polarized in the plane of incidence; but with light polarized perpendicularly to this plane, the bands occupied the same position up to a certain angle, and then with a slight increase of the angle suffered sudden displacement towards the blue, and a new band appeared near D. He also found that with light polarized perpendicularly to the plane of incidence, the position of the bands was independent of the nature of the surrounding medium, being the same when the permanganate was in air, benzene, and bisulphide of carbon; but when the light was polarized in the plane of incidence, with the increase of the refractive index of the medium the bands were more and more displaced towards the blue.

For some experiments I have made on the same subject I have used a Babinet's goniometer, which has, in addition to the ordinary horizontal stage, a vertical one so arranged that the reflecting surface can be placed over the axis of the instrument. Sunlight was used, which could be polarized in any plane by a Nicol supported by the fixed arm of the goniometer; and a small direct-vision spectroscope, by Hilger, with a "bright-point" micrometer and a reflecting prism for bringing a second spectrum into the field, was carried by the other arm of the goniometer. By placing a beaker on the horizontal stage, and, after the surface of the permanganate had been properly adjusted, filling it with the liquid and limiting the incident beam by a narrow vertical slit, the light reflected from the surface of the substance when immersed in a liquid could be examined.

The experiments were usually made with potassium permanganate crushed, and burnished with an agate on a piece of finely-ground glass; and it was found that the light reflected from the surface of crystals and from that of the substance rubbed on glass was identical; except that the blue rays were

more intense in the light reflected by the crystals, and the higher bands were more distinctly seen.

The surface-colour of potassium permanganate, and the position and intensity of the bands in the spectrum of the reflected light, are independent of the relative position of the plane of incidence to the long axis of the crystal, or to the striæ produced by rubbing, when the powdered substance burnished on glass is used.

1. *Surface-Colours.*—Freshly prepared surfaces of potassium permanganate appear of a pale yellow when light, either unpolarized or polarized in any plane, is incident upon them at low angles. But with ordinary light, and with light polarized in the plane of incidence, the amount of white light reflected is so great at high angles that the surface-colour, if any, is completely masked.

When the incident light is polarized perpendicularly to the plane of incidence, or when unpolarized light falls on the surface and a Nicol is placed between the eye and the permanganate, with its principal section in the plane of incidence, the surface-colour is seen to change as the angle increases, becoming successively green and blue, and finally white and metallic.

The surface-colours alter with the surrounding medium. The following Table gives, approximately, the colour at various incidences, (A) when the light is either unpolarized or polarized in the plane of incidence, and (B) when it is polarized perpendicularly to that plane, for potassium permanganate in air, tetrachloride and bisulphide of carbon.

Angle of incidence.		Surrounding medium.		
		Air.	Tetrachloride of carbon.	Bisulphide of carbon.
30°	A	Pale yellow.	Yellow-green.	Yellow-green.
	B	,, ,,	,, ,,	Green.
35.	A	Pale yellow.	Yellow-green.	Yellow-green.
	B	,, ,,	,, ,,	Green.
40.	A	Pale yellow.	Yellow-green.	Yellow-green.
	B	,, ,,	Green.	Green.
45.	A	Pale yellow.	Yellow-green.	Yellow-green.
	B	,, ,,	Green.	Blue-green.
50.	A	White.	Yellow-green.	Yellow-green.
	B	Pale yellow.	Bright green.	Blue-green.
55.	A	White.	Green.	Yellow-green.
	B	Yellow, with a green tinge.	Blue-green.	Blue-green.
60.	A	White.	Green.	Green.
	B	Brilliant green.	Blue-green.	Blue-green.
65.	A	White.	Green.	Green.
	B	Blue-green.	Blue-green.	Blue-green.
70.	A	White.	Greenish.	Greenish white.
	B	Blue.	,,	,, ,,
75.	A	White.		
	B	Metallic, with blue shade.		

2. *Reflection Spectra.*—With unpolarized light, and still more with light polarized in the plane of incidence, the dark bands in the spectrum of the reflected light are never very distinct. I was not able to observe whether the bands shifted or not as the angle of incidence increased, as the amount of white light reflected at angles of 55° and upwards was so great as to render the bands invisible. They appeared, however, as long as they were visible, to coincide exactly with the bright spaces in the absorption spectrum of a dilute solution of potassium permanganate, which was thrown into the field by means of the reflecting prism.

When the incident light is polarized perpendicularly to the plane of incidence, the dark bands are far more distinctly seen. At angles of less than 40° there are four bands, and the blue end of the spectrum is very weak. As the angle of incidence increases, the intensity of the blue rays diminishes; and then the amount of light in the red decreases; and at about 55° nearly the whole of the light comes from the bright bands.

As the angle of incidence increases beyond this amount, the dark bands gradually move towards the blue end of the spectrum; and at about 60° a new band appears near D. With any further increase of the angle more of the blue rays are reflected; and the bands fade away, those in the more refrangible part of the spectrum disappearing first. The relative intensity of the dark bands varies with the angle of incidence. When this is small, the third and fourth bands, counting from the red end, are darkest; with the increase of the angle the second, the first, and finally the new band, become successively darkest.

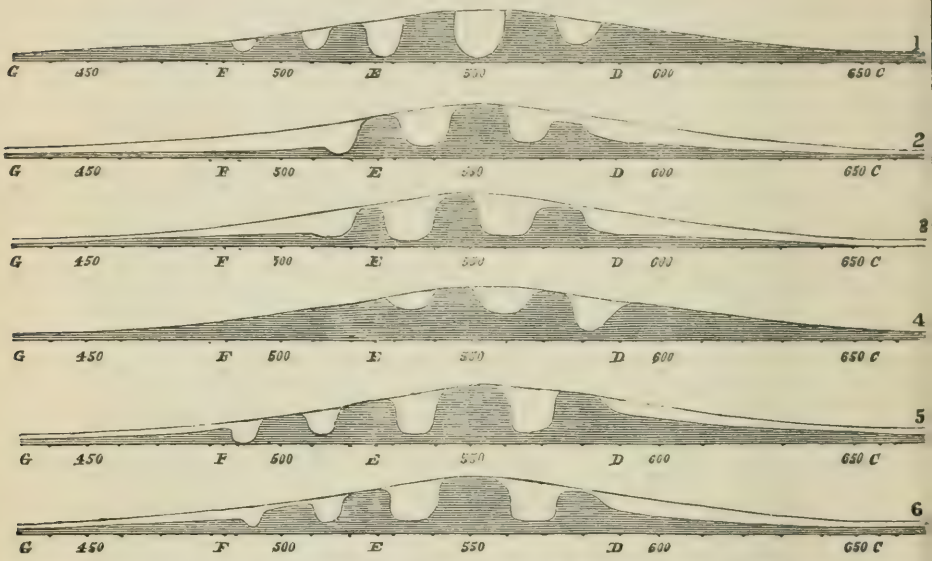
I have not been able to obtain any satisfactory measurements of the amount of the displacement of the bands, as, when a spectroscope of sufficient power to render it an easily measurable quantity is used, the bands become so ill-defined that it is impossible to measure them. Approximately the displacement amounts to about ·006 in "tenth-metres;" and the bands tend to coincide with the dark bands of the absorption spectrum, instead of with the bright bands as they do when the angle of incidence is about 55° or less.

The shaded portions of the diagrams are intended to give the relative amount of light, as determined by eye estimations, in the different portions of the absorption and reflection spectra of potassium permanganate—the ordinates being taken to represent the intensity, and the abscissæ wave-lengths. The curved line gives the intensity of the light in the different portions of the normal spectrum, as determined by Mossotti* from Fraunhofer's measurements, neglecting the minor irregularities in the curve as given by him.

* Pogg. *Ann.* lxxii. p. 509.

Fig. 1 is the absorption spectrum of a solution of potassium permanganate in water.

Figs. 2, 3, and 4 the reflection spectra, when the incident light is polarized perpendicularly to the plane of incidence, and falls on the surface at angles of 50°, 60°, and 70°.



As has already been announced by Wiedemann, the position of the bands in the reflected light depends on the nature of the surrounding medium. From the experiments I have made, it appears that, with unpolarized light, the first dark band of the reflection spectrum corresponds in position with the first bright band of the absorption spectrum, whether the permanganate is in air, benzene, or either bisulphide or tetrachloride of carbon; these liquids, however, act on the permanganate, and after a short time the surface becomes altered, and then the bands correspond with the dark bands of the absorption spectrum.

Figs. 4 and 5 represent the distribution of light in the spectrum, with fresh surfaces of potassium permanganate in bisulphide and tetrachloride of carbon, when unpolarized light is incident upon them at an angle of about 55°: in both cases the bands are wider apart than in air.

LIX. *On a possible Cause of the Formation of Comets' Tails.* *By* A. S. Davis, *M.A.**

IT is well known that the phenomena observed during the formation of a comet's tail point to the conclusion that the material which forms that appendage is being continually emitted from the head of the comet with great velocity by some force acting in a direction directly away from the sun. The material appears in most cases to be first ejected from that side of the comet's nucleus which is turned towards the sun, and afterwards, under the influence of this force, to be turned backwards to form a tail. It is the object of this paper to suggest an explanation of this force.

The remarkable identity which has been found to exist between the orbits of certain comets and the orbits of certain meteoric clouds renders it little short of certain that comets are themselves masses of solid or liquid bodies separated from one another by great intervals, except perhaps at the nucleus, where they may be closer together, and may even contain a solid core. The spectroscope indicates the presence of gas in a state of incandescence in the comet's head and nucleus; but how this incandescent gas is produced is not known.

The violent action which is observed to take place as a comet approaches the sun, on that side of its nucleus which is exposed to the solar radiation, appears to indicate that the comet consists largely of matter which is rapidly volatilized under the influence of the sun's rays.

Let us assume that such is the case, and let us consider what will be the effect of evaporation on the motion of one of the bodies undergoing it.

In the first place the mass of a comet is so small, that the force of gravitation towards the centre on any of the bodies at some distance from the nucleus must be so small that it may be left out of consideration. We know that a molecule of matter in the gaseous condition has at ordinary and high temperatures a very quick motion of translation. A molecule, as it evaporates from the surface of one of the bodies composing the comet, must acquire a velocity relative to the body of several hundred yards per second. The body must in consequence suffer a recoil in an opposite direction to that in which the molecule escapes. Now since the evaporation is caused by the sun's heat, it must take place chiefly on that side of the body which is exposed to the sun's rays. The resultant effect of all the small recoils due to the evaporation of the different molecules will therefore be to drive the body in a direction away from the sun. If the body has a motion of rotation, the

* Communicated by the Author.

whole surface might in turn become exposed to the sun's rays, and evaporation would probably take place even on the side turned away from the sun. But unless the body be of a regular shape, the effect of evaporation will be to gradually stop any rotation which it might at first have; for the force of recoil from the evaporation would act upon it in the same way as the wind does on a vane, and it would at length take up a position with its longest axis in the direction of the sun.

Now let V be the average velocity relative to the body with which the molecules escape from it. Let M be the mass of the body just before the escape of a molecule of mass μ from it. Then the velocity due to the recoil as this molecule escapes will be $\frac{\mu}{m}$ V. Now the molecules as they evaporate will start off in various directions, but almost always more or less towards the sun. Let us suppose, as being not far from the truth, that the inclination which their directions have to a straight line passing through the sun is on the average 45°; then the average velocity due to recoil acquired by the body on the evaporation of mass dm will be $\frac{dm}{m} \cdot \frac{\mathrm{V}}{\sqrt{2}}$, and the velocity acquired whilst the mass of the body is being reduced by evaporation from m_1 to m_2 will be

$$\frac{\mathrm{V}}{\sqrt{2}} \int_{m_2}^{m_1} \frac{dm}{m} = \mathrm{V} \times 1{\cdot}63 \times \log_{10} \frac{m_1}{m_2}.$$

Now the average velocity of hydrogen molecules at 0° C. is 1·06, of oxygen ·266, and of water vapour ·35 mile per second. For the sake of illustration, let us suppose that V = ·35, and $\frac{m_1}{m_2} = 1000000$, these being the values we should have to assign if the body were a block of ice containing one gramme of sand or any other non-volatile substance, the block itself being equal in mass to a cubic metre of water. The velocity due to recoil by evaporation would then be 3·42 miles per second, or about 295,000 miles per day. A tail would thus be formed which would increase in length nearly a million miles in every three days. The visible portion of this tail would consist of solid or liquid matter which had resisted evaporation; but there would also be present in the tail a large portion of the gas formed during evaporation; for since the evaporating gas has a velocity relative to the body from which it is evaporating of ·35 mile per second, those portions of the gas which have evaporated since the body acquired by recoil a velocity greater than ·35 mile per second will be also carried backwards into the tail. The estimate of the rapidity of tail-formation I have

just made has been made on the supposition that the temperature of the escaping gas is 0° C. If the absolute temperature in Centigrade units of the gas be t, we must multiply the above estimate by $\sqrt{\frac{t}{273}}$. Now in no case has the rapidity of tail-formation greatly exceeded one million miles in three days, except in the case of those comets which have approached very near to the sun, and where, consequently, the temperature at which the evaporation has taken place must have been very great. Donati's comet is one of the most striking examples of comets with large and rapidly formed tails which have not approached very near to the sun; and in Donati's comet the tail increased in length from 14 million miles on August 30 to 51 million miles on October 10, or at an average rate of somewhat less than a million miles in a day.

Those comets which have formed large tails with exceptional rapidity have approached very near to the sun. Thus the comet of 1680, which formed a tail 60 million miles long in two days during its perihelion passage approached so near to the sun as to be exposed to a solar radiation 25,600 times more intense than that to which the earth is exposed. To use Sir John Herschel's words, "In such a heat there is no solid substance we know of which would not run like water, boil, and be converted into vapour or smoke." It seems probable that it is only the shortness of the time during which the comet is exposed to such a temperature which prevents its being altogether converted into vapour. The smaller fragments of the comet will, I conceive, be entirely evaporated; and the last portions of vapour from any fragment will, as I have shown, be carried backwards with immense velocity into the tail. After this vapour has arrived in colder regions, it seems probable that it will condense and become visible as a cloud of finely divided solid or liquid matter. In these cases then the visible tail will consist, not of matter which has resisted evaporation, but largely and perhaps almost entirely of matter which has evaporated and has recondensed.

In conclusion, if this be the true explanation of the phenomenon of comets' tails, then every meteoric cloud of matter which approaches sufficiently near to the sun to undergo rapid evaporation must become tailed like a comet, as it passes through its perihelion passage. I would suggest that we may have here an explanation of the radiated structure of the sun's corona. As different masses of meteoric matter approach close to the sun, the smaller fragments will be almost or entirely evaporated, a large portion of the vapour from them being carried rapidly away from the sun, thus giving rise to a

coronal protuberance. Thus the radiated structure, and the irregular and variable form of the corona would be accounted for.

Cheltenham College,
October 21, 1878.

LX. *Notices respecting New Books.*

The Theory of Sound. *By* JOHN WILLIAM STRUTT, BARON RAYLEIGH, *M.A., F.R.S., formerly Fellow of Trinity College, Cambridge.* Volume II. London: MacMillan and Co. 1878. 8vo, pp. 302.

WE noticed the First volume of this work shortly after its publication (5th ser. vol. v. p. 66). The Second volume—which, we presume, completes the work*—is now before us. We could not, perhaps, give it higher praise than to say that it is worthy of its predecessor. Not to speak of actual contributions to our knowledge of the Theory of Sound which have been made by the author, it is scarcely possible to overestimate the value to the student of a perfectly trustworthy work which brings together the substance of memoirs scattered through a variety of periodicals. Not fewer than about a hundred and twenty or thirty memoirs are referred to in the course of these volumes, most of which would be inaccesible to students living away from the chief centres of intellectual activity. These, however, are not the only students who are benefited by such a work as the present. Even those who are more favourably situated rarely look at the memoirs unless they are distinctly interested in their subjects at the time of publication; and this is particularly the case with memoirs on such a subject as the Theory of Sound, the mere reading of which may involve a considerable expenditure of time and labour.

The subject of the present volume is Aerial Vibrations. In its general method it resembles its predecessor. Thus, in the former volume, the discussion of Vibrating Systems in general (chap. iv. and v.) is preceded by a very careful consideration of a particular case, viz. that of a system having one degree of freedom (chap. iii.); so, in the present volume, the discussion of the general problem of vibrations in three dimensions (chap. xiv. and xv.) is preceded by that of the cases of Vibration in tubes (chap. xii.), and of some other special problems, including the Reflection and Refraction of Plane Waves (chap. xiii.). These four chapters, together with an introductory chapter on "Aerial Vibrations" (chap. xi.), fill more than half the volume. The remainder is divided into chapters on the Theory of Resonators (chap. xvi.), on Applications of Laplace's Functions to Acoustical Problems (chaps. xvii. and xviii.), and on Fluid Friction (chap. xix.).

* The Work, as it stands, might certainly be accepted as a complete treatise on what is generally understood by the Theory of Sound, viz. the Kinetics of Acoustical Vibrations; and we should have supposed the work to be complete had not the publisher made himself responsible for an announcement of Vol. iii., and also for a notice as to volumes subsequent to the first.

The seventeenth and eighteenth chapters are almost wholly occupied with contributions made by the author to the Theory of Sound. Thus the seventeenth chapter does, indeed, begin with a long extract from Professor Stokes's paper, "On the Communication of Vibrations from a Vibrating Body to a Surrounding Gas," in which he applies his determination of the complete value of ψ (the symbol which represents a disturbance propagated wholly outwards) to the explanation of "a remarkable experiment by Leslie, according to which it appeared that the sound of a bell vibrating in a partially exhausted receiver is diminished by the introduction of hydrogen" (vol. ii. p. 207). The explanation of this seemingly paradoxical phenomenon, it may be remarked, had escaped the penetration of Sir J. Herschell, who "thought that the mixture of two gases tending to propagate a sound with different velocities might produce a confusion resulting in a rapid stifling of the sound" (p. 214, vol. ii.). So far the contents of the chapter are due to Professor Stokes; the remainder is taken from two papers by the author published in the 'Proceedings of the Mathematical Society'—"On the Vibrations of a Gas contained within a Rigid Spherical Envelope," and an "Investigation of the Disturbance produced by a Spherical obstacle on the Waves of Sound."

The eighteenth chapter contains a discussion of considerable interest from a mathematician's point of view, viz. "a Proof of Laplace's Expansion for a Function which is Arbitrary at every point of a Spherical Surface." But to put this in a proper light we must look back to vol. i., where a proof is given of Fourier's Series. The method adopted may be indicated as follows:—The author first considers the motion of a vibrating string when the ends are not absolutely fixed—a state of things which he represents by supposing a mass (M), treated as unextended in space, attached to each end and acted on by a spring (μ) towards the position of equilibrium,—and then particularizes his solution in two ways, *first*, by supposing $M=0$ and $\mu=\infty$, so that the ends of the string are fast; *secondly*, by supposing that both μ and M are zero, a case which might be represented by supposing the ends of the string capable of sliding on two smooth rails perpendicular to its length. From the results thus obtained Fourier's Theorem is shown to follow. In connexion with this proof, the author remarks:—"So much stress is often laid on special proofs of Fourier's and Laplace's Series, that the student is apt to acquire too contracted a view of the nature of those important results of analysis" (p. 159, vol. i.); and he adds, in a note, that "the best system for proving Fourier's Theorem from Dynamical considerations is an endless chain stretched round a smooth cylinder, or a thin re-entrant column of air inclosed in a ring-shaped tube" (p. 160, vol. i.).

It will be observed that the remark above quoted implies a promise of a similar discussion of Laplace's Series; and this is fulfilled in chap. xviii. The "system" adopted is that of a thin spherical sheet of air. In chap. xvii., as we have seen, there is a discussion of the vibrations of a gas contained within a rigid sphe-

rical envelope ; and it is observed (p. 238) that a similar treatment will apply to the vibrations of air between two concentric spherical envelopes ; but when the difference between the radii is very small in comparison with either, the problem reduces itself to that of a spherical sheet of air. The case in which the velocity-potential ψ is symmetrical with reference to the poles, is treated first ; and it is shown that it can be represented by a series whose general term is

$$P_n(\mu)\left\{A_n\cos\frac{at\sqrt{n(n+1)}}{c}+B_n\sin\frac{at\sqrt{n(n+1)}}{c}\right\},$$

where n is an integer even or odd, and $P_n(\mu)$ Legendre's function. If now $t=0$, ψ is an arbitrary function of the latitude, and we see that

$$\psi=A_0+A_1P_1(\mu)+\ldots+A_nP_n(\mu)+\ldots$$

This is, of course, only a particular case of Laplace's Series. But by similar reasoning on the general value of ψ Laplace's Series is established. At each step of the process the case is considered in which the radius of the sphere becomes infinite, and we pass physically to the case of a plane layer and analytically from Laplace's to Bessel's functions. "The vibrations of a plane layer of gas are of course more easily dealt with than those of a layer of finite curvature ; but I have preferred to exhibit the indirect as well as the direct method of investigation, both for the sake of the spherical problem itself with the corresponding Laplace's expansion, and because the connexion between Bessel's and Laplace's functions appears not to be generally understood" (p. 265, vol. ii.).

This discussion is, as we have already remarked, of purely mathematical interest ; and, indeed, from the nature of the case, by far the largest part of the work is addressed to mathematicians. Here and there, however, are discussions of which the interest is purely physical, such as, in the present volume, that on Whispering Galleries (p. 115), that on the Refraction of Sound by Wind (p. 123), and others. But our limits will not allow us to do more than mention their existence.

LXI. *Intelligence and Miscellaneous Articles.*

A FEW MAGNETIC ELEMENTS FOR NORTHERN INDIA.

BY R. S. BROUGH.

HAVING recently had occasion to measure the dip of the needle and the strength of the horizontal component of the earth's magnetic force at Calcutta, Jubbulpore and Allahabad, with a view to ascertaining to what extent the indications of an arbitrarily calibrated galvanoscope uncorrected for the local value of the earth's magnetism, would be trustworthy, I think it desirable to put the results on record.

The horizontal intensity was measured with a Kew-pattern portable unifilar magnetometer; and the observations have been corrected for temperature, torsion and scale error.

Stations.	Longitude.	Latitude.	Date.	Horizontal force in dynes.	Dip.
Calcutta	88° 22′ 50″	22° 32′ 32″	Jan. 1878	0·37158	28° 59′ 30″
Jubbulpore ..	80 00 00	23 10 00	Dec. 1877	0·36667	29 23 30
Allahabad	81 54 12	25 27 43	Dec. 1877	0·35915	33 18 45

Dividing the horizontal component by the cosine of the dip, we obtain the total force thus:—

Calcutta . 0·42482 dyne.
Jubbulpore . 0·42084 „
Allahabad . 0·42977 „

There are on record several observations of the dip in Calcutta, which it will be interesting to bring together here.

The dip appears to have been measured for the first time when the French corvette 'La Chevrette' visited these waters in 1827, by M. de Blosseville, who found it then to be*

26° 32′ 38″.

Ten years later, in 1837, on the occasion of the visit of another French corvette, 'La Bonite,' to the Hugli river, the dip was measured at Kalagachia (Diamond Harbour) by the chief Hydrographer, who found it to be†

26° 39′ 04″,

exhibiting a change of only 0° 06′ 26″ from the result of the earlier measurement.

The next and most recent measurement was made by the brothers Schlagintweit in March 1856 and in April 1857, in which years it was found to be respectively‡

28° 06′ 43″
and 28° 22′ 56″.

The same observers found the dip at Jabalpur in December 1855 to be§ 28° 31′ 08″

Their measurements of the horizontal force gave:—

0·37386 dyne at Calcutta in March 1856,
0·36644 „ „ in April 1857,
0·39959 „ at Jabalpur in December 1855.

A very valuable series of observations was made in 1867-68 by the late Captain Basevi, R.E., under the orders of Colonel J. T. Walker, C.B., R.E., Superintendent of the G. T. Survey (now Surveyor-General of India), at 14 stations, extending from 15° 6′ to 30° 20′ north latitude‖; but none of them are coincident with the three stations under consideration.

The values of the dip and horizontal intensity at the limiting stations of the series were as follows:—

* Asiatic Researches, vol. xviii. part i. p. 4.
† Proceedings, Asiatic Society of Bengal, Wednesday, 3rd May, 1837.
‡ 'Observations in India and High Asia,' vol. i. § *Loc. cit.*
‖ General Report of the Operations of the Great Trigonometrical Survey of India during 1867–68.

Stations.	Latitude.	Longitude.	Date.	Horizontal intensity.	Dip.
Namthabad	15° 06′ 00″	77° 36′ 00″	April 1868	0·37401	11° 40′ 56″
Deyrah	30 20 00	78 06 00	Jan. 1867	0·33604	41 27 34

—*Proceedings of the Asiatic Society of Bengal*, February 1878.

ON MOLECULAR ATTRACTION IN ITS RELATIONS WITH THE TEMPERATURE OF BODIES. BY M. LÉVY.

The demonstration which we have given, in our last communication, of a general law upon the dilatation of bodies rests on the two fundamental propositions of thermodynamics, and upon this other proposition—that the mutual actions of the molecules of a body are independent of their temperatures.

This last proposition we have assumed as an hypothesis; we wish now to prove that it flows from the first proposition of thermodynamics, so that our law itself will be found to be built *solely* upon the two propositions which serve as a foundation for that science.

To justify this assertion, let us conceive any body in motion under the influence of:—(1) external forces, F; (2) mutual actions, f, on the nature of which *we will make no hypothesis*; (3) a certain quantity of heat received from without.

Let $d'Q$ be the quantity, positive or negative, of heat received during an infinitely short interval of time dt (we will employ the characteristic d' for the infinitely small quantities which are not exact differentials or which are not known *a priori* to be so): a portion $d'q$ of this heat is employed for increasing the temperatures of the various points of the body; the surplus, or $d'Q-d'q$, is transformed into work, and gives rise to a quantity of work $E(d'Q-d'q)$, E being the mechanical equivalent of heat.

Suppose that the body describes any complete cycle, which means not only that all its points describe closed curves and resume their velocities at the end of the orbit, *but also that they resume their temperatures*. If to this cycle we apply the theorem of the *vires vivæ*, we get

$$0=\int \Sigma \mathfrak{T}_e F+\int \Sigma \mathfrak{T}_e f+E\int d'Q-E\int d'q,$$

$\mathfrak{T}_e$ denoting an elemental work.

But, in virtue of the first proposition of thermodynamics,

$$\int \Sigma \mathfrak{T}_e F+E\int d'Q=0,$$

whence

$$\int(\Sigma \mathfrak{T}_e f-Ed'q)=0, \quad \ldots\ldots\ldots\ldots \quad (a)$$

which is equivalent to saying that the quantity under the symbol $\int$ is the total differential of a certain function *of all the variables*, which resume their values at the end of the cycle—that is, not only of the coordinates x_i, y_i, z_i of the various points of the material system considered, and which we suppose to be n in number (so that $i=1, 2, 3\ldots n$), but also of the temperatures T_i of those

points. Thus

$$\Sigma\mathfrak{T}_e f - \mathrm{E}d'q = -\mathrm{E}d\mathrm{U}, \quad \ldots\ldots\ldots\ldots \quad (b)$$

U being a function of the $4n$ variables x_i, y_i, z_i, T_i. This function is no other than that which is called the *internal heat*.

The equation (a), or its equivalent (b), is the only one that can be directly deduced from the first proposition of the mechanical theory of heat, if no preconceived idea on the nature of heat be admitted; and we do not understand the reasonings by which some have attempted to deduce from it that $\Sigma\mathfrak{T}_e f$ is a differential. It has certainly been proved that, for certain *particular* cycles, during which the temperature or the quantity of heat received remains invariable, we have $\int \Sigma\mathfrak{T}_e f = 0$; but from this it is not permissible to conclude that $\Sigma\mathfrak{T}_e f$ is a differential.

I now say that, *whatever idea may be formed of the nature of heat*, the quantity of heat $d'q$ employed to raise the temperatures of the various points of the body, *without displacement of those points*, is necessarily the exact differential of a function of the n variables T_i.

In fact, the quantity of heat necessary for raising by $d\mathrm{T}_i$ the temperature of a molecule of mass m_i is necessarily an expression of the form $m_i\gamma_i d\mathrm{T}_i$, as γ_i can only depend on the temperature T_i of the molecule and on the specific constants relating to the material of which it is composed.

Therefore the total quantity of heat remaining in the sensible state is

$$d'q = \Sigma_i m_i\gamma_i d\mathrm{T}_i = d\Sigma m_i \int \gamma_i d\mathrm{T}_i;$$

$d'q$ being thus a differential, so also is $\Sigma\mathrm{T}_e f$, in virtue of (a); and as this sum is an expression of the form

$$\Sigma_i(\mathrm{X}_i dx_i + \mathrm{Y}_i dy_i + \mathrm{Z}_i dz_i)$$

containing no term in $d\mathrm{T}_i$, it cannot but be the differential of a function not containing the variables T_i, consequently containing only the coordinates x_i, y_i, z_i.

It follows from this:—first, that molecular attractions admit a function of the forces; secondly, that this function remains the same whatever may be the temperatures of the various points of the body; and, thirdly, that consequently the mutual action of two molecules of a body is quite independent of the temperature—which completely justifies the law laid down in our last communication, and places it among the necessary consequences of the two propositions of thermodynamics.

That law, that the pressure of a body heated under constant volume varies linearly with the temperature, proves that the empiric definition of temperature adopted by Dulong and afterwards by Regnault, viz. the pressure of a gaseous mass with constant volume, might be easily extended to the case in which, instead of a gaseous mass, any other body was in question.

Finally, without wishing here to draw from this law all the consequences which it admits of, we will nevertheless make the following remark:—

In a previous communication we have sought to discover what are the data *strictly* necessary to be derived from experiment to enable one to study a body from the thermodynamic point of view;

and the importance of this question will be especially apparent if we observe that in the best treatises superabundant data are taken from observation, even for constructing the simplest theory of all (that of gases). We then arrived at a result which can in brief be enunciated thus:—To know all the isothermal lines of a body, and one of its adiabatic lines, is sufficient.

The law which forms the object of the present investigation conducts to the following much more satisfactory and quite unexpected result:—*In order to know all the isothermal lines and all the adiabatic lines of a body*, and consequently to be able to study it completely, *it is necessary and sufficient to know* two *of its isothermal lines and* one only *of its adiabatic lines.*

In physical terms, one may say that it is sufficient to observe:—1st, the dilatation of a body under *two* different pressures, or, more generally, for two series of states answering to two curves arbitrarily traced in the plane of the (pv)'s (which is equivalent to saying that the ∞^2 observations, of which we spoke at the outset of our previous communication, are replaced by two simple infinities of observations); 2ndly, one of the specific heats, or *one* particular pressure *only*, or, more generally, for *a single* series of states of the body corresponding to a curve arbitrarily traced in the plane.

If we admit, with MM. Clausius and Hirn, that the thermal capacity of every substance is a constant, this second series of observations reduces itself to a single observation.—*Comptes Rendus de l'Académie des Sciences*, Sept. 30, 1878, t. lxxxvii. pp. 488–491.

THE SONOROUS VOLTAMETER. BY THOMAS A. EDISON, PH.D.

The sonorous or bubble voltameter consists of an electrolytic cell with two electrodes—one in free contact with a standard decomposable solution, and the other completely insulated by vulcanized rubber except two small apertures, one of which gives the solution free access to the insulated electrode, and the other allows the escape of bubbles of hydrogen as they are evolved by electrolysis. With a given current and a given resistance a bubble is obtained each second, which is seen at the moment of rising, and which at the same time gives a sound when it reaches the air. The resistance may be reduced so as to give one bubble in one, five, ten, or fifty seconds, or in as many hours. I have compared this instrument with the ordinary voltameter, and find it much more accurate. By the use of a very small insulated electrode and but one aperture, through which both the gas and water current must pass, great increase of resistance takes place at the moment when the bubble is forming; and just before it rises, a Sounder magnet included within the battery-circuit opens, closing again when the bubble escapes, thus allowing by means of a Morse register the time of each bubble to be recorded automatically. This apparatus, when properly made, will be found very reliable and useful in some kinds of work, such as measuring the electromotive force of batteries &c. By shunting the voltameter and using a recorder it becomes a measurer, not only of the current passing at the time, but also of that which has passed through a circuit from any source during a given interval.—Silliman's *American Journal*, November 1878.

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